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THE

NEW-YORK

MEDICAL AND PHILOSOPHICAL

JOURNAL

AND

REVIEW.

VOL. II.

NEW-YORK:

Printed and sold by *T. & J. Swords,* Printers to the Faculty of Physic
of Columbia College, No. 160 Pearl-Street.

1810.

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Clerk of the District of New-York.

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MEDICAL AND PHILOSOPHICAL
Journal and Review.

VOL. II.—NO. 1.

For the Medical and Philosophical Journal.

OBSERVATIONS *on the* PATHOLOGY *of wounded* ARTERIES.

By JOHN B. DAVIDGE, *M. D. Joint Professor of Anatomy, Surgery, and Physiology in the Medical College of Baltimore. Communicated to* J. AUGUSTINE SMITH, *Professor of Anatomy and Surgery in the University of the State of New-York, and Member of the Royal College of Surgeons, London.*

LETTER I.

DEAR SIR,

TO your polite favour I should have returned an earlier answer, but for a disability of my right hand, occasioned by a slight and transient casualty. That you "will derive pleasure" from a correspondence with me, is both flattering and gratifying; but when you demand "improvement," you will permit me to admonish you to make your drafts on other banks than those over which I have control, lest, from a deficiency in credit on my part, your bills return dishonoured.

The *nosology* of a wounded artery is interesting and highly important. Its range in consequences, in all serious, indeed trivial wounds, is great, and well entitled to the attention of the surgeon.

You will not, my friend, be startled at the sound of the word *nosology*, the object of which is, an inquiry, you know, into the *nature*, the *symptoms*, and *consequences* of diseases; except, indeed, you have become a convert to the new philosophy of the day—except you consider nosology as an unfit subject for the present enlightened period, believing that in dark periods of ignorance, when gross night overshadowed the schools, such stupid themes had an apology in the dull, unimproved state of the times. But now that a beam of new light has awakened the medical world from its long and deep slumber, you may imagine that a subject more ennobled by novelty, would be less unbecoming. Be it so; and let my present mention of it find an excuse in my early prejudices. But I widely miscalculate if I do not in some little speculation, which, if I can rouse myself from my indolence, I intend to send you, *demonstrate* that where nosology ends, medicine ceases to be a science. But to the subject of the present letter, lest you think that I have designedly strayed to hunt up the errors of great men.

A sword passes through the fore-arm, and some small arteries, with a larger one, the radial, ulnar, or inter osseous for instance, are divided. The blood pours furiously out for a time, but is, by coagulation of the effused fluid, and the coarse means externally applied by the family, or some generous neighbour, stanched. The surgeon arrives, and finds all things quiet, although in homely order. He feels reluctant to open anew the vessels by breaking up the dressing; and begins to hope and persuade himself that the hæmorrhagy will not recur. How vain and illusory!

In a day or two the hæmorrhagy breaks out again, and is again checked; and thus I have seen matters proceed, for two and three weeks, until the hand, wrist, and fore-arm have been completely injected, and the patient exhausted to the extremest degree. And what does all this arise from? From one of three things.

The surgeon has read, or heard, for all our surgeons do not read, that if the lips of the wound in an artery be coaptated, whether by good luck or artificial means, they will unite after the manner of a vein, the gluten being poured out by the vasa vasorum. Or, if the artery be completely divided, it will contract up to its first branch. Or, a coagulum without and within, will form, and serve as a stopple to the artery. And in this last, countenanced by the doctrine of the vitality of the blood, he begins to imagine to himself, or talk to those about him, how this coagulum forms its new vessels, and how they stretch out towards the walls of the artery, and how, by uniting with the sides, the plug is secured. Here are the three sources of error in the surgeon, and danger to the patient.

I hold it as an axiom in the surgery of a wounded artery, that no hæmorrhagy from an artery will cease, except the sides come into close contact and union.

In the smaller arteries, I conceive, that, by the effused and coagulated blood without and about the sheath of an artery, its sides are forced into union; that is, that they come into contact; that the vessels of the internal coat are excited, and take on that action which brings about what surgeons call the union by the first intention.

What is it in those smaller vessels which, after the patient is put to bed—his heat restored—and the re-action established, causes the recurrence of the hæmorrhage? Plainly and palpably the increase of the current of blood forcing open the mouths of the divided arteries, in opposition to the mechanical pressure without; not the heat or any other cause relaxing the contraction of the extremity of the arteries.

And what is it in the larger arteries which causes them again and again to pour out their blood? To my understanding, the putrefaction and melting down of this coagulum, to which the surgeon unhappily trusts. And as often as the coagulum forms, the hæmorrhage is stopped; and as often as it begins to putrify, and melt down, and fall away, the hæmorrhage returns. If there be a coagulum within at all, it is too insignificant to be noticed; and, it will ultimately deceive the surgeon. I do not speak from conjecture; nor do I rest on the highly respectable authority of Mr. John Bell, who appears to be destined, in his luminous course, to give light to the universe of surgery. But, as I had the pleasure of mentioning to you, I have had an opportunity of dissecting two stumps, at the distance of thirty odd days from the period of operation. In which, when the vessels were neatly and fairly exposed, they were found completely cylindrical and open down to one eighth of an inch of the very extremity. The wall that closed up and formed the end, was but very little, if any, thicker than the sides measured at any place above in the most healthy parts.

Here was no coagulum, no plug, no conversion into ligament up to the first branch. But the lumen arteriæ was open and free down to the very point where the ligature closed upon the walls. This dissection was nearly forty days from the period of operation, and yet the artery had all the characters of health—its coats having no marks of thickness in any part.

There were several medical gentlemen present during this dissection, and among them my ingenious and learned colleague, Dr. Cocke; a gentleman well known to you for his politeness, suavity of manners, and intimate knowledge of the healing art; and whose mind, we may suppose, was well prepared to detect any circumstance that might militate against the doctrines of the schools in which he had been educated—those of London and Philadelphia; and in those schools the doctrines of contraction and coagulation have certainly not been without friends.

But from what I have advanced you are not to infer, that I deny the possibility of a union of the sides of the vessel, up to the first branch. I admit it; but not from contraction, nor coagulation, but from the walls of the artery being forced into contact by mechanical agency; and this would happen not only up to the first branch, but up as high as the mechanical force acted.

At first, immediately on tying of the vessel, there may be, and really is, a coagulum within—yet this soon melts down, and is mixed with the common circulating mass. And here the surgeon is deceived. He takes the column of jelly, in a few weeks to be melted away, for living substance, filling up the tube.

Yourself and every other gentleman accustomed to the knife, knows, from repeated observation, that when an artery is divided, it retracts, and draws itself up within its cellular sheath; but the contraction of a divided artery even in a slight degree is to be conceded with caution. If an artery is to be provoked by adventitious stimulus, it will contract; and this so long as its excitability shall be exposed to foreign stimulation. But it is not on this that the surgeon is to depend. It is uncertain, irregular, and of small compass.

When an artery is divided, lies exposed, and has its caloric with the fluid, which fills its vasa vasorum, to fly off, it is possible that its canal may be diminished below the natural state of its systole; though this is too unimportant to be brought into account; and I am induced to consider the contraction of an artery an imaginary thing, equally illusory with the coagulum within the artery, the plug or stopple of which surgeons delight to speak.

Mr. John Bell says, "that an artery, in the moment of being divided, may be made to re-unite, I hold to be an absolute fact, not because it is proved by the authority of those who have prevented aneurisms by a skilful compress; for in such cases the aneurism being prevented, we are still left in doubt, whether the artery was actually wounded! but the aneurismal varix ascertains the fact." vol. i. page 211.

Most singular indeed! that an aneurismal varix ascertains the fact, that the lips of a wound in an artery adhere, if laid well together at the moment the artery is wounded. Or should we not say, that of all facts, the aneurismal varix gives the most decided proof that the wound has not united: for, in the aneurismal varix, the lips of the wound of the artery adhere to the fascia below, while those of the vein adhere to the fascia above, by means of the intermediate cellular substance. If the lips of the artery were united, there could no longer exist an aneurismal varix, as the current of blood from the artery which constitutes the disease, would be cut off by the closing of the aperture in the artery. Even Ulysses would not at all times pass the arrow through the ring, were Minerva to leave his side.

My opinion is, that we should secure every injured artery by regular compress or ligature, doing by art what must be done by accident, if the hæmorrhage cease; for I do not hesitate to lay it down as a grave and serious fact, that the walls of every artery must come into contact, and its lumen be obliterated, when a wound in an artery is successfully managed. I am fully aware, that many, and able surgeons, are of a different opinion; and that arteries are kept to show the gelatine up the tube, and consolidation up to the first branch. This gluten, no doubt, with all its appearance of commencing organization, would have been gradually carried away, had the patient lived but a few weeks. The serum and red globules, we all know, quickly run off, and after a time, as assuredly, the more tenacious or viscid gluten is dissolved. This we see when clots of blood are laid on healthy granulating wounds—when long compressed in the uterus, &c.

And the consolidation up to the first or second branch, is equally obvious. The walls are in contact; a slight action, approaching to that of inflammation, takes place, and a cord of more or less thickness, is left to deceive the surgeon.

The case, so frequently referred to, and so much confided in by surgeons, by Lambert, of Newcastle upon Tyne, is far from deciding even the possibility of the re-union of the lips of a wounded artery, much less that it is to be generally expected.

When in a numerous series of experiments, one only is to appearance successful, we should be extremely careful how we extend our confidence. I am, in all those solitary instances of success, much inclined to doubt, and feel it my duty to guard myself from deception, by critically and severely examining into the circumstances of the case.

There are against Lambert's case many weighty objections. He tells us, and upon his words I shall take the affair, that, "a small steel pin, rather more than a quarter of an inch long, was passed through the two lips of the wound in the artery, and secured by twisting a thread round it, as in the hair lip. This was found to stop the bleeding," &c.

Whether the discharge of blood was arrested by the pin, with the thread about it, approximating the lips, and wholly closing the aperture in the artery, is to me very questionable. I rather conceive it to have been from the bulk of the pin and thread pressing, so as mechanically to close up the diameter of the vessel. And this pressure was undoubtedly increased by the binding up of the arm: for in a wound so extensive as to expose the brachial artery at the fold of the elbow, the consequent inflammation and swelling could not have been inconsiderable. The pressure must increase momentaneously with the inflammation and tumefaction. And, in fact, if at first the anterior wall of the artery was not in contact with the posterior, it must have been so about the fourth day, the period at which "the wound was first drest."

This, my conjecture, is highly corroborated, by the fact of the pin coming away with the dressings on the fourteenth day. Here it is certain that the anterior part of the artery must have been affected with high inflammation, and even gangrene and sloughing, or the pin could not have come away with the dressing. It otherwise would have remained for the surgeon to draw out, having previously disengaged the thread. It is clear, and beyond a question, that the front of the vessel mortified and sloughed. Can you conceive it probable, that in a vessel so small, and pressed on as this must have been, the anterior portion of its circle could inflame, gangrene, and fall away, and yet its lumen to be preserved, and a current of blood to continue down through it? If so, your conception is more sprightly and ingenious than mine.

But the surgeon says, that "the pulse was very little altered immediately after the operation," "and so strong and equal, that he had no doubt of the blood continuing to circulate ſreely through it." What! is there no way but the great trunk for the blood to get into the radial and ulnar arteries? Are there no anastamosing branches which might possibly convey it down, and thus support the pulsation of the radial or ulnar artery? I have seen, and any surgeon may see when he pleases, the upper portion of the temporal artery beat freely and strongly when its trunk below has been absolutely cut asunder by the shoulder of the lancet; not surely by a continuance of the circulation through the trunk, but by the return blood through the anastamosing branches.

Superadded to the chance by the collateral branches, is it not possible, and indeed highly probable, that in this solitary case of success, the great trunk may have divided, high up in the arm, and of consequence a large portion of the blood would go off, and thus support the pulsation below?

That the diameter of the artery was completely annihilated, all the circumstances of the case tend to prove. And as to the pulsation below, I can readily conceive it to be kept up by the circulation through the anastamosing branches, as already remarked, on the possible lusus naturæ, the branching off, high up the arm, of the radial trunk. And had the arm upon the death of the patient been injected, we have every reason to believe the canal of the artery would have been found obstructed. Vide Med. Observ. vol. i. p. 360.

Here is another circumstance of arterial hæmorrhagy, which at present solicits my attention. I allude to the frequent recurrence of hæmorrhage after the operation, for the popliteal aneurism, when the operation is performed in the front of the thigh. There is nothing in the nature of a femoral artery aneurismal in the ham, which should make it subject to hæmorrhagy. The fault, without doubt, must be solely in the manner of securing the vessel. I need not enter minutely into detail of the various methods of doing this plain affair—the ligatures of reserve—the metallic plates—the modes of confining the loops, &c. They all tend more or less to break up the connections of the vessel with the surrounding parts. They deprive it of nourishment, by cutting off its channels of support. The very solicitude to secure the vessel, together with its being left continuous and undivided, has been the source of all the evil.

What could be requisite to produce hæmorrhagy in an artery, more than to have it insulated—cut off from all the neighbouring parts—bedded in pus and coagulated blood? Surely nothing. How could its private œconomy be maintained, when the very instruments of support were destroyed, when the blood could no longer flow to its bare and exposed walls?

And after John Bell had written so ably and intelligibly about a plain fact, which, to be understood, needed only to be pointed out, how could he suffer his transcendent pen to encumber itself with that mongrel equivocal philosophy, that so unintelligibly discourses of "the artery being eroded by the foul pus?" The idea of the sides of an artery being eroded by foul pus, is so far below that clear and luminous philosophy which so vividly shines through every part of Bell's great work—the greatest of surgical works—that I can scarcely consent that it shall stand in the splendid copy I possess.

What more need he have said, than that the vessel was insulated, and without support? Consequently its walls must die, mortify, and fall away; and thus would the blood burst out. To Bell we owe much in many things, especially for what he has written on the arteries. To what he has said about the manner of managing the affair, I can add nothing.

Baltimore, July 25, 1809.

LETTER II.

Dear Sir,

When I did myself the pleasure of addressing to you my former letter, on the pathology of wounded arteries, I had not seen the work of Dr. Jones on that subject, and all I knew of the work, I collected from the desultory conversation I had with you. Not having seen the work, I could have no specific objects against which I might direct my observations. The principles of Jones I conceived to be erroneous, and, at the moment, offered a few reasons, on which I thought them inadmissible; I further added to those reasons, in my letter to you, and now having, by your courtesy, the work before me, I shall present, in full, my objections to his doctrines.

The Doctor has managed his subject with much ingenuity; his language is easy, perspicuous, and rises to elegance; the plan and order of his experiments are natural and unexceptionable; but I cannot grant my approbation to the manner in which he has found it convenient to represent the various results of his numerous experiments. With the results, as they stand, I am particularly gratified. Nor have I the slightest wish to deduct from their force. I admit them in all their influence; but I cannot agree to the interpretation the Doctor has put upon them. I must assume the right of inquiring into, and minutely weighing the experiments in their circumstances; I expect to show a preponderance different from that given by the Doctor, to the public. Nothing is more palpable to my understanding than that, in his laboured work, Jones has ascribed more importance and efficiency to some of the circumstances than can justly be attributed to them; and that he has overlooked the more immediate relations between them and the results.

If I can, without offering violence to the general tenor of the facts, or shading any one part to the disadvantage of another, satisfactorily explain to you that the Doctor has not been happy in his deductions, I shall consider myself fortunate; but yet I shall do no more than justice to my undertaking.

In some place of Bacon's elaborate work he has remarked that, in the explication of any phenomenon, more causes than are adequate and necessary should not be adduced. This grave apothegm is the suggestion of a cautious and circumspect philosophy; a philosophy luminous and observant. This caution arose out of a clear conviction, furnished by years of accurate and attentive observation, and it should ever be kept in view by those who may come before the public, engaged either in the illustration of old principles, or the establishment of new hypotheses.

There is nothing more usual in the institution of experiments, for the ascertainment of any fact, than for the experimenter to enter on the affair with his mind more or less bent up to his purpose. In his closet he anticipates what he wishes to find in his experiments, and in his experiments he is sure to discover what he has already anticipated. We first enlist our prejudices and affections in behalf of our doctrines, and then the illusions of the circumstances not unfrequently flatter us, that our experiment has afforded the clearest verification of our hypothesis.

I do not premise this, to excite any undue prepossession against the deductions drawn from the experiments of Dr. Jones; far from it: I wish these experiments to be open to a fair analysis and honourable discussion.

I would not trouble you with this criticism, had not Dr. Jones laid open anew the avenues of error, which had been so judiciously narrowed, and in a great degree closed by Ponteau and Bell. For, although to surgeons of experience and accurate observation, to say that we are to trust to spontaneous contractions of divided arteries; or to clots blocking up the canal of an artery; or finally and permanently mending an aperture in the side of a punctured vessel, would be unproductive of serious consequences; yet to minds untrained in the school of practice, those reveries of the closet serve as most fatal lessons. Upon the first authority, I can assure you that we are not without example in this city of the application of this doctrine of spontaneous contraction and natural coagulation, to the great disappointment of the surgeon, and fatal result to the patient. Indeed, to me this return to the obsolete doctrines of Petit and Kirkland, is little better than the revival of the old affair of the wonders wrought by stypticks, &c. upon the altar of which so many thousand lives have been immolated. But that I may not appear to wish to preoccupy your mind in behalf of what I am about to say on the experiments of the Doctor, I will present them, in their prominent and most characteristic features, to you, along with my remarks.

To be particular, which I intend to be, I must unavoidably be a little prolix; too much so for a letter; but the importance of the subject must serve as my apology.

Preparatory to the discussion of the main points more immediately the subjects of consideration, it may not be amiss to premise a few observations, on the structure of arteries, and also on their general function, viz. their process in receiving and distributing the blood. On this general function, the Doctor has been rather silent.

" The substance of which arteries are composed is divided into distinct parts," says the Doctor, " which have been called tunics or coats. Three coats, which have received various names, can be readily demonstrated, and may be simply and clearly distinguished by the terms, internal, middle, and external.

" The internal coat, although extremely thin, is very close in its texture, and gives to an artery a smooth and polished lining; it is elastic and firm, considering its delicate structure, in the longitudinal direction, but so weak in the circular as to be very easily torn by the slightest force applied in that direction. The morbid changes which have been observed in it, prove that this coat is vascular, and some experiments have been related to show the probability of its being sensible.

"The middle coat, which is thickest, is formed by numerous layers of firm, compact, fleshy fibres, of a pale red colour, passing in a circular direction, but appearing rather obliquely connected and interlain with each other, than forming complete circles. These fibres are of a peculiar nature, are well supplied with nerves, and resemble, in form and disposition, muscular fibres, but differ from them in possessing a remarkable degree of elasticity. Their elasticity keeps a dead artery open and circular; for this coat, when detached from the internal and external coats, still preserves the cylindrical form, whilst they, on the contrary, in a state of separation, become flaccid, and collapse. As this coat has no longitudinal fibres, the circular fibres are held together by a slender connection, which yields readily to any force applied in the circumference of the artery. The middle coat is intimately connected with the internal and external by a very short and fine cellular membrane.

"The external coat, anatomically considered, is so simple, that many authors have thought it sufficient to say, that it is formed of condensed cellular membrane, which, by becoming gradually of a looser texture, connects the artery with the surrounding parts; but the importance which is attached, in a surgical view, to this coat, renders a more particular account of it highly necessary and interesting. Although ultimately resolvable into cellular membrane, yet it derives from the particular arrangement of its component fibres, a characteristic appearance, which distinguishes it from cellular membrane, and entitles it to be ranked as a proper coat of an artery. Internally, or next to the middle coat, its texture is close and smooth; externally, more open and rough, in consequence of the cellular membrane by which it is connected with an additional covering. The whole is remarkable for its whiteness, density, and great elasticity. *If an artery be surrounded by a ligature, its middle and internal coats will be as completely divided by it as they can be by a knife, whilst the external coat remains entire;* a fact which will be commented upon in another part of this treatise, and shown to be connected with important circumstances. The strength, therefore, of an artery depends chiefly on its external coat, which answers in some respects, the purpose of a strong fascia."

The above quotation I consider to be necessary, not because you and I could not recollect what anatomists have advanced on the structure of arteries, or what we ourselves have demonstrated it to be, but because, being in language peculiar to, and admitted in doctrine by Dr. Jones, I can the more easily show that his physiological and pathological observations are not always on the best terms.

According to its structure, as above developed, if an artery be wounded transversely, the edges of the wound recede to a certain distance from each other; if it be wounded longitudinally, the lips of the incision in the same manner, for the fibres of the middle coat are nearly circular, retire from each other, leaving a longitudinal chasm; and if an artery be completely divided in its transverse direction, the extremities retract, the one towards the breast, the other from it, to a considerable distance; but the diameter continues the same, for the "elasticity of the fibres, circular in their direction, of the middle coat," we are assured by Dr. Jones, "keeps a dead artery open and circular."

In page 114, Dr. Jones, detailing the immediate effects of wounds of arteries, as they respect the form and appearance of such wounds, tells us that "the longitudinal appears to produce the slightest possible, or perhaps scarcely any separation; the oblique occasions a separation proportioned to its extent; and the transverse, however small, seems to produce a circular aperture in the parietes of the artery." And in note C, refers us to an opinion of John Bell, expressed on a case of Deschamp, which decides a principle.

Have the circular fibres of the middle coat no power or action? When the circle is violated by the slit-like cut up and down the wall of the artery, what prevents the fibres of the middle coat from acting? Will they not retract and gather up on themselves? They have the same elasticity that the longitudinal fibres have. I am astonished at the facility with which Bell has suffered himself to be led astray. With Dr. Jones there is more ample apology; he is evidently, from his writings, not a practical surgeon. Do we not see, too frequently, in unhappy operations with the lancet in the common affair of abstracting blood from the arm, that in those slit-like wounds the lips do not remain in complete contact and apposition? In operating on the basilick vein, if we penetrate so deep as to touch the artery, we for the most part make the incision pretty nearly parallel with the longitudinal fibres of the humeral artery, descending through the fold of the elbow, just before it divides into the ulnar and radial trunks. And yet who ever understood that such wounds would not open in obedience to the elastic power of the circular fibres of the middle coat? Is there no separation of the lips in those longitudinal cuts, when a new wave of blood is impelled along, forcing the artery into a state of diastole? Surely, my friend, hypothesis has some share in this grave doctrine.

But to put all conjecture to rest, I have seen, and demonstrated, in a living artery, a longitudinal incision to the extent at least of half an inch: and in this the edges receded very considerably from each other, and exposed, in a great degree, the opposite internal surface, although the vessel was kept in an extended state, thus mechanically favouring the approximation of the edges, and counteracting the natural retraction of the circular fibres. Six or eight medical gentlemen witnessed this.

Were I not writing to Professor Smith, I would take into account, in my behalf, the opinion of Monro the father, in his great work, where he says, that if an artery be but slightly wounded, the edges instantly retract, and have an aperture as if an artist's punch had passed through.

But this error, as to the state of the lips of a longitudinal cut in the parietes of an artery, so directly in the teeth of the anatomy and obvious physiology of the artery, is not the only thing of this quotation I have to notice. I have also to call your attention to the effects of a ligature thrown round an artery. The Doctor says, that "if an artery be surrounded by a ligature, its middle and internal coats will be as completely divided by it as they can be by a knife, whilst the external coat remains entire." If this position, which the Doctor has dignified with the rank of a fact, is intended to convey to the mind of the reader the idea that this division of the internal and middle coats is the usual consequence of a ligature applied in the ordinary way by a surgeon, and it can have no other surgical import, I am far from admitting it. It is, in my view, altogether gratuitous, and not deducible from any data furnished by the Doctor's experiments. But I shall postpone all further consideration of this until I come to the experiments instituted for its illustration. I now advance to the experiments of the first chapter.

CHAPTER I.—SECTION II.

Experiment 1.—"The hæmorrhage did not cease (from the carotid of a horse) before its death. *The circular membrane surrounding the divided portions of artery was very much filled with blood;* both extremities of the artery were considerably *contracted*, particularly the one next to the head, but there was no coagulum," &c.

Experiment 2.—"The animal bled to death, &c. the truncated extremities of the artery had retracted about an inch and a quarter, and were very much *contracted*, but their mouths were open; *the arterial sheath and cellular membrane were much distended with blood*," &c.

Experiment 3.—The cut extremities of the artery were separated nearly an inch; both appeared to be *very much contracted; the surrounding cellular membrane and sheath were very much filled with blood*," &c.

It were a waste of time to recapitulate in detail, all the particulars of the nineteen experiments of this second section. It may suffice to say that in each one, the above prominent facts were fully ascertained, viz. *the contraction of the mouth of the divided extremity, and injection of the cellular membrane, surrounding the artery and sheath, with coagulated blood and lymph*, and the mutual recession of the ends of the separated vessel.

There can be no dispute about the retraction of the extremities of a divided artery, therefore I shall pass on to the other points.

By every stroke of the heart a wave of blood is urged forward into the artery, by which the artery is dilated, or is forced into what is termed its diastole; the elasticity of the tunics of the artery being by this wave of blood called into operation, for the coats are put into a state of distention, restores the artery from this state of dilatation to its ordinary quiescent state, or condition of systole; and in this condition of rest or systole it would continue, were it not again altered by a succeeding wave of blood from the heart. In this play of systole and diastole consists the pulsation.

It is necessary here not to mistake this systole of the artery, this natural quiescent state, for what the Doctor calls the *contraction;* for if the artery were dead and dissected out of the body, and laid by itself on a table, it would retain this systolic condition. Elasticity is equally the property of living or dead matter. The Doctor wishes to convey a far different idea to the mind of his reader; he is desirous to impress him with the persuasion that the artery acted on by some unexplained laws, or spontaneous causes, is, by way of provision against approaching ruin, able to contract, and really does contract the mouth of the severed extremity.

The experiments uniformly present to us two points; the one is the reduction of the diameter of the end of the artery, or what Dr. Jones terms the *contraction;* the other is the infarction or engorgement of the cellular substance surrounding the artery. Now if this infarction or injection of the circumjacent cellular substance is fully adequate to the explanation of the reduction of the diameter, or lumen of the artery, why should we be over tenacious of this *contraction?* why should we so solicitously look out for the support and maintenance of this creature of the imagination?

When the arterial sheath is injected with blood, and the surrounding cellular membrane much filled with lymph, as the most pointed testimony of each experiment unequivocally establishes, either this cellular membrane surrounding the vessel must be enlarged, forcing apart and displacing all the superincumbent fat and muscle, and causing the volume of the limb, if it be in an extremity, to be increased; or the parietes of the artery must be urged into smaller compass, viz. the diameter of the artery must be lessened. The arterial tube is empty, or if filled, it can only contain coagulated blood, a material soft and yielding, and readily giving place to a force from without.

Can rational, enlightened minds hesitate a moment about this alternative? It is not more plain that two and two amount to four in common arithmetic, than that if there be an internal force of one thousand, operating against a counteracting force of five, the internal force still accumulating, that the counteracting force of five must give way.

From the nearest and best view I can take of the subject, I am convinced that the cellular membrane surrounding the artery is highly injected; that the end of the cut artery is reduced in its diameter; but this is done mechanically, and not by any spontaneous contraction in the vessel itself; nor do I see any insurmountable impediment to this conclusion, except it be the very respectable authority of Dr. Jones, who, with all the serious gravity imaginable, assures us, in the eighteenth experiment, that " there was a considerable layer of coagulated lymph, in which the artery was completely enclosed, but *not compressed* by it, although it was impossible to conceive a more complete engorgement of the cellular membrane surrounding it." And, to enforce with additional energy the doctrine of the text, our author adds, in a foot note, " I make this observation because the state of this artery forms an insuperable objection to Ponteau's doctrine, that the suppression of hæmorrhage depends on the engorgements of the cellular membrane." Then the Doctor does verily believe that, "although the portion of artery next to the head was so completely surrounded by coagulated lymph, that it was impossible, from an external view of it, to say exactly where it terminated," the whole force of this accumulated and accumulating lymph spent itself in displacing the superincumbent fat and muscles, and even putting the integuments themselves on the stretch, without at all compressing the parietes of the artery. Had Ponteau laboured seven years he could not have hunted up an experiment more to his purpose. And to what state of the artery Jones can possibly allude when he says it forms an insuperable objection to Ponteau's doctrine, I cannot conjecture. If the subject were not the adjustment of an important point of science, I should believe the Doctor was sporting with the easy faith of his reader.

The conical state of the extremity of the divided artery we also deduce from this coagulated blood and lymph compressing the sides. Why it is conical is very obvious; the coagulation immediately at the point is greater than it is higher up, consequently the extreme end is more diminished in its volume than farther on.

In section the third of the same chapter, Dr. Jones proceeds to show that we are not only to expect, in the natural process for the suppression of arterial hæmorrhage, a contraction of the ends of the vessel; but, in addition to this spontaneous contraction, that we are uniformly to meet with an external and internal coagulum of blood, together with a coagulum of lymph interspersed between those sanguineous coagula.

No surgeon of any reading or observation can hesitate to admit the existence of an *external* coagulum of blood; it is met with on every examination, and in every surgical writer. We cannot open a fresh wound without seeing it. The evidences however, of the uniform occurrence of an internal coagulum are not so great, and so generally within the reach of our senses, nor of that genuine, irrefragable sort which are to be collected from the faithful history of unadulterated cases. The coagulated lymph, on which the Doctor lays so much stress, is certainly to be found, but when he undertakes to show that it serves as a coagulum, or stopple, to close up the tube of the artery, he undoubtedly becomes too mechanical in his ideas; but more of this presently.

In conceding the presence of an *external* coagulum in the sheath, and attached to the mouth of every recently divided artery, provided the artery be not of the larger class, I must not be understood as granting also its permanent utility. Its advantages are transient and illusory. By mechanically filling up the sheath left vacant by the retraction of the extremity of the vessel, and attaching itself, by means of the cohesive property of the coagulable lymph incorporated with it, to the end of the artery, but no sooner does this coagulum begin to liquify, and melt down, than this whole mechanical support gives way, and a new dash of blood disquiets the patient, and alarms the surgeon.

I have seen this coagulation about the mouth of the vessel, this suppression of hæmorrhage, and this liquifaction and falling away of the mechanical transient support, again and again occurring, and it will constantly overtake the steps of the surgeon so long as he shall pursue the deviating track which keeps in sight the permanent aid of this external coagulum.

An *internal* coagulum of blood is not always to be met with; so rarely indeed does it occur, that many of the best and most experienced surgeons have searched for it in vain, for it has been a moot point nearly an hundred years. Nature in her processes, whether acting by the co-operation of many means, or simple causes, is, and must be uniform: a thing that is contingent is not of the regular process of nature.

White, in his cases, says, "I next laid open the arteries to their extremities, and found them entirely closed (sides completely in contact and consolidated) near an inch from the end of the stump; but from that point upwards *their capacities were not at all diminished*, nor was there any *coagulum or clot of blood* in the vessels or any where near them."

Ponteau remarks, that he " very seldom found any traces of internal coagulum; that in four examinations, one of a stump of an arm eight days after amputation; two of legs, one ten days, and the other three weeks after the operation; and of a thigh four weeks after amputation, he found no coagulum."

It is very extraordinary, if this internal coagulum be a part of the process of nature, that those two able and observing surgeons could discover nothing of it in those five or six cases. But Dr. Jones, anxious to break the force of these cases bearing so immediately on this disputable point, anticipates the conclusion of the reader, by reminding him that artificial means were resorted to. It is true they were; but will Dr. Jones, or any other gentleman for him, demonstrate that this artificial aid has prevented, or can prevent such a process, if it be a work of nature? I confess that in this suggestion of the Doctor, there is, in my opinion, more of evasion and subterfuge, than argument. What can be the difference between the subsequent operations of the extremity of an artery when a ligature or compress is used, and when this extremity is compressed by a coagulation round the parietes, thereby forcing them into contact?* The external coagulum as readily forms in the artificial suppression as in the natural, and the effusion of lymph is the same; for the effusion of lymph and the external coagulation are totally independent of the means, whether natural or artificial, made use of for the suppression of the hæmorrhage. The effusion of the lymph is produced by the excitements in the exhalents, caused by the disturbance in their economy by the cutting instrument, and the coagulation is a mere property of dead matter, for a time acting in the coagulable lymph or gelatine: and the effusion of lymph is uniform, internally as well as externally, but it is not seen after a few days, because it is partly taken up by the absorbents of the internal coat, and partly carried away by the circulating fluid. But to proceed.

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* But Dr. Jones does not admit this compression. *Editors.*

" After he (a large horse, the subject of the experiment) was killed," says Gooch, " we dissected the thigh, and found that the bleeding was not suppressed by coagulated blood, but by the vessels being close contracted (their walls mechanically compressed) for near an inch or more from their extremities."

" When hæmorrhage stops of its own accord," adds John Bell, " it is neither from the retraction of an artery, nor the constriction of the fibres, nor the formation of clots, (internal coagula of Petit) but by the cellular substance, which surrounds the artery, being injected with blood." Here Bell repeats to us the doctrine of Ponteau, which is firmly established by the experiments of Dr. Jones.

On this opinion however, of John Bell, Dr. Jones indulges in the following criticism : " We have therefore only to discuss the difference between a *clot of blood* and *coagulated blood*, to discover the principal difference between this offspring of Mr. Bell, and what he has been pleased to call Petit's sickly child."

From respect to Dr. Jones I would not say that he has willingly misrepresented Bell, but most unquestionably he has egregiously misunderstood him. When Bell says, " nor the formation of clots," he, to the understanding of every man, plainly conveys the idea of the *internal* coagula of Petit, for against Petit's doctrine he is contending, and cannot be supposed to have in his view the *external* coagulation or injection of the surrounding cellular membrane, which is the true and real cause of the suppression. Dr. Jones, in his next edition will, I hope, leave both his candour and understanding unincumbered by suspicion.

That there is now and then an internal coagulum, I admit to be probable, yet I cannot grant it to be any part of the process of nature for the suppression of an arterial hæmorrhage ; when it occurs, it can only be considered as a casualty, transient in its consequences, and of no permanent duration.

On the division of an artery, there is, at first, a dash of blood, but, if the artery be small, the effusion gradually lessens until the blood poured out shall coagulate, at first at a distance, and by degrees it approaches the mouth of the vessel, and finally closes its tube by bringing the sides into contact. The irritation occasioned by the wounding instrument, after a few hours, causes the exhalents, both without and within the canal of the artery, to throw out an increased quantity of lymph; for the artery, in fact, inflames: the sides being compressed from without, are forced into contact and coaptated like the lips of a wound, a thin stratum or almost imperceptible quantity of lymph is effused between the parietes of the vessel, through which fine arteries, with all the apparatus of organization, pass, mutually receive each other, or anastamose, and ultimately constitute a continuous organized substance. Thus do the living walls, by these intermediate vessels, become permanently united; the blood circulates from side to side, and nature's grand object is attained. But yet, like an artist who, when he has completed his building takes down his scaffolding, nature, by apparatus of her own, gradually removes this effused lymph from the inside, from between the tunics, and from without, and the whole artery, down to the coalesced points, is restored to its healthy natural appearance and condition. The effused lymph serves merely as an appropriate bed for the tender vessels to generate in, and no sooner is the organization accomplished than this now useless matter, is carreid away by the industrious absorbents. Thus stood the fact in the arteries dissected by myself, referred to in my former letter; thus stood the fact in the cases of Ponteau; and thus stood the fact with the accurate and observant White. Nor in my opinion will the careful dissector ever find it otherwise if he waits until the whole process of nature shall be gone through. As to internal columns of blood or lymph either becoming in and of themselves organized; sending their vessels out to meet those of the sides, and ultimately constituting with the sides an organized living part of the animal system, it is a flight of imagination against which I seriously and warmly object.

Whether the sides of an artery close by a single line, as was the case with my subject, or are united for half an inch or two inches, it matters not; in either case the union depends on the sides being more or less pressed into contact, and kept so by external agents, or the cohesive property of the lymph, until they shall firmly grow together.

So far as respects partially divided or punctured arteries, I delivered my sentiments in my former letter, and in Dr. Jones I meet with nothing that alters in the slightest degree my opinion as you then received it. I am not conversant with the pathology of wounds in the arteries of the lower animals, but according to the success of the Doctor in brutes, the anatomy between their wounds and similar ones in the human kind must be very different; at least, if the testimonies of surgeons are to be received.

In the third chapter, Jones treats of the immediate effects of the ligature, used I suppose in the ordinary way, of surgeons. He herein endeavours to show the direct consequences of the ligature to be a total separation of the internal and middle coats of the vessel. Early in his work he announced as a fact this consequence of the ligature, and this chapter is pretty much devoted to illustrate it by apposite experiments. The whole that I can gather from the most attentive perusal of the experiments is, that, when a ligature was applied round the vessel, and in a short time thereafter removed, there was found an effusion of lymph, sometimes in the form of a transverse septum cutting the upper from the lower part.

These experiments, as they are detailed, are extremely inconclusive, and, to my mind, quite unsatisfactory. To them there are several things to object. We, in the first place, have to ascertain whether, in the application of the ligature to the artery of the brute, more force was not exerted than is ordinarily used by surgeons; in the second place there was no demonstration that these coats were divided at all, it remains a thing of inference from the effusion of lymph; and conceding, which we do not, that a separation of the fine vessels necessarily preceded the effusion, it does not follow that the immediate effects of the ligature was the division of the internal and middle coats, for the ligature might mechanically so affect the susceptible constituent parts of these tunics as to induce inflammation, gangrene, and sphacelus of the whole circle within the loop. Every surgeon knows that the circle within the loop uniformly dies and falls away; but this is a secondary operation, and does not take place until after the sides, immediately above the edge of the mortifying circle, have united and become firm.

But, in the third place, my chief objection is, that when a ligature is applied, even but for a short time, the interruption to the circulation in the vasa vasorum of the internal and middle tunicks, and the bruising they receive, must excite more or less of irritation, which quickly runs on to inflammation. In this state of excitement, innumerable facts assure us, that the exhalents pour out a vast quantity of coagulable lymph. Nor is it requisite that the integrity of the fine vessels should be violated, either for the production of the lymph, or absolute union of the surfaces. We could instance the copious effusion of lymph, along the internal surface of the trachea, in cases of the croup; also, in inflammations of the pleura, the numerous occurrences of a permanent union of the pleura costalis with the pleura pulmonalis; and to these may be superadded the various attachments between the several duplicatures of the peritoneum in cases of abdominal inflammation.

The Doctor certainly would not argue that, because the lymph was effused, or because internal surfaces coalesce, there necessarily must be a division of vessels or an ulceration of surfaces.

I now close my long, and, I am apprehensive, uninteresting letter. I have endeavoured to be plain and intelligible in all its parts: I have objected to no opinion that I did not think seriously objectionable; and I have in no place quoted Dr. Jones's sentiments, so as, by dissociating them from the general text, to make them appear to a disadvantage. I hate disputation, and I abhor a war of words.

Baltimore, October 27, 1809.

Whatsoever may be the process employed by nature in the suppression of hæmorrhages, we entirely agree with Dr. Davidge, that the surgeon should trust to nothing but the ligature (where it can be applied) in wounds of arteries capable of endangering the life of the patient. But we by no means think that the doctrine of Petit and Jones more calculated to mislead the surgeon, than that of Ponteau and Bell, (leaving the correctness of both out of the question) for he would be equally blameable who trusted to external compression from injected cellular membrane, &c. as he who relied upon internal coagula, for the former do not lay more stress upon the efficacy of coagula, than the latter do upon that of compression.

Whether the internal coat of an artery is always cut when a ligature is applied around the vessel, is more than we can say; perhaps it is not in the old method of taking up arteries with the needle, in which much of the surrounding substances is included; but when an artery is secured with the tenaculum, this coat, from its extreme delicacy, is probably divided. The facility with which it may be done can be easily seen by making the experiment upon a piece of dead artery.

We believe with Dr. Davidge, and we think that Scarpa has proved it incontestibly, that a wounded artery never heals in the human subject without a destruction of its canal. *Editors.*

For the Medical and Philosophical Journal.

An uncommon Case of MAL-FORMATION, *in which an unsuccessful Operation was performed. By* J. AUGUSTINE SMITH, *Professor of Anatomy and Surgery in the University of the State of New-York, and Member of the Royal College of Surgeons, London.*

ON the 9th of June, 1808, I saw the child of —— Jesse, living at Free-Port, Gloucester county, Virginia. The child was about seven months old, and allowed by all who saw it to be uncommonly large, fat, and healthy in appearance for one of that age. The account I received from the parents was as follows: That soon after birth it was taken very ill, and, upon examination, the anus was found imperforate. Some medical gentlemen saw the child, but attempted nothing material for its relief. It was relieved, however, in an extraordinary manner, for it was observed that something yellow was discharged with the urine, which no doubt was the meconium. In this way has all the fœcal matter been discharged ever since. The child, as may be expected, is liable to severe fits of retention of urine, when the urethra becomes obstructed with fœcal matter of rather thicker consistence than usual; he has hitherto, however, been uniformly relieved by his own exertions. But as his life is always in imminent danger from a fatal obstruction, which must certainly happen whenever he eats any thing solid, if not before, and as it was uncertain where the rectum terminated, whether in the urethra or in the bladder, or, if in the latter, in what part of it, I thought an operation might relieve, and ought to be attempted. On examination, the raphè was found to terminate near where the anus ought to have been, which place was distinctly marked by a deep brown colour. I fixed the patient as if I were going to operate for the stone, having previously introduced a catheter as a guide, and made the incision to the left of the raphè, but lower down than in lythotomy, and carried it through the middle of the discoloured spot directly towards the point of the coccyx. A ligamentous substance in appearance was discovered to occupy the place where the anus ought to have been. Having cut through this, I came to cellular membrane; dissecting upwards and backwards so as to avoid the vasa differentia and vesiculæ seminales. I frequently introduced my finger to ascertain if I could feel the rectum forced down by the struggles of the child, but in vain. In this manner I went on, distinctly feeling the catheter in the bladder, but unable to discover any intestine, although I introduced my finger as far as it would reach; when, thinking it unsafe to thrust my knife any further, I was obliged, though unwillingly, to desist. Thus my operation was unsuccessful. What the state of the parts may be I am in hopes to discover when the child dies, the father having agreed it may be opened. A case similar to the foregoing is related by Roonhuys, as cited by Hiester,* but no further notice is taken of it.

B. Bell† mentions such cases generally, without specifying any in particular. Where the rectum cannot be found with the knife, he recommends thrusting a trocar in the direction the gut usually takes. I had a trocar ready, but as my finger must have reached to the superior part of the sacrum, I would not use it. As to cutting into the abdomen to search for the intestine, I take that to be a slip of the pen.

15th. The child recovered in a few days from the operation, and enjoyed its usual health.

It died in about a month afterwards, in consequence of eating some potatoe, but the mother would not consent to its being examined, as I was afterwards informed, for it had been buried some time before I heard of its death.

Since the foregoing was sent to the press, I have been informed that a similar case occurred during the last summer in this state.

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* Vide Hiester's Surgery, vol. ii. page 244.
† System of Surgery by B. Bell, vol. ii. page 275 et seq.

For the Medical and Philosophical Journal.

An ACCOUNT *of a Case of* LUMBAR ABSCESS, *which proved fatal, although the* VERTEBRÆ *were unaffected. By* MOSES LITTILL, *Student of Medicine. Communicated to* J. AUGUSTINE SMITH, *Professor of Anatomy and Surgery in the University of the State of New-York, and Member of the Royal College of Surgeons, London.*

DAVID LANGDON, aged 22 years, a shoemaker by trade, of a delicate constitution, was several times affected with catarrh in the years 1805 and 1806. In 1807 he had pneumonia, which was followed by hectic fever, and in the latter part of the same year he had a rupture in the right groin, which was cured by a truss. In the beginning of 1808 he was attacked with pain in the loins, which extended to the thigh. Three months after a tumour was discovered on his back, and shortly after another in the left groin, which was enlarged on coughing; the tumour on the back extended from the right os innominatum all over the left gluteus maximus. He had hectic fever and night sweats; and diarrhœa came on about six weeks before his death, with soreness and pain in the breast, frequent vomiting, so that he could take but a small quantity of food at a time; the vomiting and diarrhœa gradually increased until death, accompanied with swelling of his arms and legs. The tumour was punctured nine days before his death in the most dependent part, which was about one inch and a half from the trochanter major, with a trocar, in an oblique direction: about ℥xv. of reddish matter were evacuated, and the tumour in the groin then disappeared. The orifice was dressed with lint and emp. adhesivum; it healed in four days. He did not appear to be any way affected by the evacuation. He took wine bitters, (for he could not take bark) elix. vitriol, and astringent mixtures.

Examination after death. The intestines adhered to the peritoneum, so that it required considerable force to separate them; the liver adhered to the diaphragm, and it was also greatly enlarged; the spleen was also a little enlarged, and there was a small collection of water in the abdomen. The abscess was found to extend internally from the second vertebra of the loins to Poupart's ligament, and externally from the spine, and right innominatum to half of the left innominatum; the abscess was under the gluteus maximus muscle, and contained about ℥ iv. of matter, the vertebræ were sound. He died June 29th, 1809, and was examined a few hours after death.

CASE *of* CAROTID ANEURISM. *By* ASTLEY COOPER, *Esq.* *F. R. S. Surgeon to Guy's Hospital.*

From the Medico-Chirurgical Transactions.

IN the year 1806 I had the honour of presenting to this Society an account of an operation for carotid aneurism, which terminated unsuccessfully.

I then took the liberty of observing, that I thought that the result of that case ought to have no influence in preventing a similar operation under more favourable circumstances, as the death of the patient was attributable to the advanced stage of the disease at the time of the operation. The aneurismal tumour had become so large as to press upon the air tube, and occasion cough, difficulty of breathing, and even effusion of coagulable lymph upon its internal surface; and it had also compressed the pharynx, in a degree to prevent even the passage of fluids into the stomach.

I had no apprehension of the functions of the brain sustaining any permanent injury from a ligature on the carotid artery, having the evidence of Dr. Baillie to prove, that one carotid had been entirely obstructed, and the diameter of the other considerably lessened in the same person, without any apparent ill effects. I have also given a drawing in my former paper, of the left carotid artery being obstructed by the pressure of an aneurism of the aorta; and we have the analogy of animals to show, that both carotids may be tied without any visible effect upon the functions of the nervous system. I had many years ago made the experiment of tying these vessels in the dog, and immediately as it was concluded, the animal was placed upon the ground and showed no diminution of voluntary powers. It exhibited the same fears and affections as before, and betrayed no greater loss of appetite, or disposition to sleep than an animal usually does who has been the subject of an experiment which gives some pain, but does not injure the vital functions. A preparation made from this dog has been long preserved in the collection at St. Thomas's Hospital; and the parts have been injected to show the principal vessels which carry on the circulation, which are the two thyroideal arteries, and two branches from each vertebral, passing under the angles of the lower jaw.

I was therefore resolved to take the first opportunity of repeating the operation before the disease had advanced so far as to interrupt, in a considerable degree, any other function than that of the passage of the blood through the carotid artery; and my friend Mr. George Young, surgeon, had the kindness to furnish me with a case of the most favourable nature, by sending, first to my house, and afterwards to Guy's Hospital, a man who was in all respects the subject I should have chosen. He had a mind cool and firm, yet obedient to every injunction: a body equally unirritable as his mind—the tumour only about two inches and a half in diameter, and the neck of considerable length, so as to give ample space for exposing the carotid artery. The history of this case is as follows:—

HUMPHREY HUMPHREYS, aged fifty, who has been employed to carry loads of iron* as a porter, observed six or seven months ago, a tumour, having a pulsatory motion, and about the size of a walnut, on the left side of the neck, just under the angle of the jaw, and extending from thence downwards to the thyroid cartilage. It was accompanied with great pain on the left side of the head, which began about five months ago, and was attended with a sense of pulsatory motion in the brain. The tumour affected his speech, so as to make him extremely hoarse; and he had more recently a cough, attended with slight difficulty of breathing, and which seemed to be the effect of the pressure of the swelling on the larynx. His appetite was sometimes affected by it; for three or four days he eat heartily, and then for many lost his relish for food. He had a sense of coldness succeeded by heat in his left ear, and he often became sick when eating, but did not vomit. Upon attempting to stoop at any time from that period, he had an insupportable feeling, as if his head would burst; a giddiness; loss of sight; and almost total insensibility.

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* The employment consists in this:—A collar of wood is placed around the neck and upon the shoulders, and he carries bars of iron on each shoulder, thus protected.

The left eye, which had for some time been gradually closing, appeared now not above half as large as the right; yet its power of vision was equally perfect.

A blister was at this time ordered to be applied on the head by Dr. Hamilton, which lessened his pain. A month ago he applied another with the same relief; but it lasted only for a few days. He continued at work until the day previous to the operation.

The dilatation of the carotid artery was seated just below the angle of the jaw, and about the acute angle which is made by the great division of the common carotid. The tumour was about the size of a pullet's egg, and prominent in its middle.

The pulsation of the aneurism on the day of the operation was remarkably strong; when the sac was emptied by pressure on the artery below, the tumour sprang to its original size with one contraction of the heart.

I proposed to tie the common carotid below the dilated part, and the operation was performed at one o'clock on the twenty-second of June, 1808, at Guy's Hospital.

I began my incision opposite the middle of the thyroid cartilage, from the base of the tumour, and extended it to within an inch of the clavicle, on the inner side of the mastoid muscle. On raising the margin of this muscle, the omo-hyoideus could be distinctly seen crossing the sheath of the vessels, and the nervus descendens noni was also exposed. I next separated the mastoid from the omo-hyoideus muscle, and the jugular vein became apparent, which being distended at every expiration spread itself over the artery. Drawing aside the vein, the par vagum was evident, lying between it and the carotid artery, but a little to its outer side. This nerve was easily avoided.

A blunt iron probe, constructed for the purpose, was then passed under the artery, carrying a double ligature with it. Two ligatures being thus conveyed under the artery, the lower was immediately tied. I next detached the artery from the surrounding parts to the extent of an inch above the lower ligature, and then tied the upper. Lastly, a needle and thread were passed through the artery above one ligature and below the other. The division of the artery was then performed.

Nothing now remained but to dress the patient, and this was done by drawing the parts together by adhesive straps, the ligatures hanging from each end of the wound, and by laying on a piece of lint retained by straps of adhesive plaister.

Mr. Vose, my dresser, (whose attention to the case was unremitted, and to whose care and knowledge many of my patients have been indebted for their recovery) now asked the patient if he experienced any unusual sensations about his head. He answered, that for the first time since two months after the formation of the tumour, he was relieved from a distressing pain which extended up the left temple, accompanied by a violent throbbing of all the arteries of that side. This pain never returned.

The pulsation in the tumour, however, had not entirely ceased, although it was so much diminished as to become obscure; but it was felt by my colleague Mr. Forster, by Mr. George Young, Mr. Dubois, jun. from Paris, who accompanied Mr. Young, by Dr. de Souza, and many others who were present at the operation. I concluded it to be the effect of the return of blood by the internal carotid artery from the brain, in consequence of the free anastomosis which exists between the blood vessels within the skull.

The patient was put to bed, with his head elevated, and in this position he felt quite comfortable.

Three p. m. Pulse was moderate, skin cool, suffered very little pain. Pulsation in the tumour perceptible, but inconsiderable when contrasted with its force before the vessel was tied.

Five p. m. Pulse stronger and fuller, but in other respects as before; head entirely free from pain.

Eight p. m. Patient's pulse reduced to the healthy standard, skin cool; says he feels no pain.

June 23.—*Six a. m.* Patient passed a good night.

One p. m. I saw the patient; he had a slight cough; has had no evacuation since the operation; pulse was not quicker than natural.

Ten p. m. The patient got out of bed, and went to the water-closet, and had an evacuation.

June 24.—*Six a. m.* Pulse natural; pulsation in the tumour continues; tumour sore when compressed; has become firm, for the blood which was fluid in it prior to the operation and all yesterday, is now coagulated; pain, and a sense of fulness felt on the right side of the head.

June 25.—*Six a. m.* Patient says he no longer feels pain in any part; has had a good night; has only one troublesome symptom, viz. an occasional rattling in the larynx from accumulated mucus; pulse this morning quite temperate.

Three p. m. The tumour is considerably diminished; pulse moderate; no constitutional irritation.

June 26.—*Eight a. m.* Patient had a good night; pulse still moderate; skin cool.

Eleven p. m. Still free from any disagreeable symptom.

June 27.—*Seven a. m.* Patient very restless during the night; coughed much and had pain in the head; spirits depressed; pulse natural.

Half past one p. m. Pulse eighty-four; feels much better than in the morning; has had an evacuation from the bowels since last night.

June 28.—*Seven a. m.* Pulse natural; had a tolerable night; bowels open; no pain.

One p. m. I saw the patient; pulse eighty-four; slight pulsation still to be felt in the tumour, which is much diminished.

June 29. Pulse natural; no pain; pulsation still perceptible; tumour so much less that the skin is wrinkled over it.

June 30. Wound dressed the first time, and has united by the first intention as far as the ligatures would permit; he is free from irritation.

July 1. Pulse natural; man tranquil; pulsation very obscure; tumour firm; he is very hoarse.

July 2. No stool; ordered opening medicine; very hoarse, so as to speak only in a loud whisper.

July 3. Pulsation doubtful; man healthy.

July 4. Going on well.

July 5. Wound looks well; man appears natural; but the hoarseness continues.

July 6. He is free from any symptoms of irritation.

July 8. Patient says the tumour is now only half its size at the time of the operation.

July 9. Ordered a poultice.

July 12. Ligatures projecting more; and much more discharge from the wound.

July 14. Upper ligature came away, being removed by Mr. Vose.

July 15. Lower ligature came away; pulsation very obscure.

July 17. Man walked out of his ward; the tumour at this period was reduced to less than half its size. The pulsation in it was with difficulty perceived; but it continued until the beginning of September, at which period all who saw him agreed that the pulsation had ceased, and the tumour was then scarcely apparent. The facial and temporal arteries on the left side cannot be felt.

The wound was a long time in healing, first from a sinus in the course of the ligatures, and afterwards from a fungus where the sinus had been placed.

The man was discharged cured, on the 14th day of September, and returned to the occupation of a porter at Crawshay's iron wharf, Thames-street.

The result of this case afforded me a degree of pleasure which compensated for the disappointment I felt in the issue of the former. In a professional point of view, it was highly desirable to ascertain the possibility of saving life in a case which had hitherto proved generally fatal; and I could not but feel more than common interest in the fate of a man, who, although he well knew that the trial was new, and the risk considerable, never betrayed the smallest signs of apprehension.

Near eight months have now elapsed since the operation was performed, and he has returned to his former employment without any diminution of his mental or corporeal powers, excepting the lessened action of the temporal and facial arteries on the side in which he was operated. The tumour has disappeared, and he has not been since subject to that pain in the head, by which he had been so much distressed prior to the operation.

This aneurism, from the depth of its situation, was, I believe, seated in the internal carotid artery, and this led me to hope that the regurgitation of the blood, although at first sufficient to produce a slight pulsation in the tumour, would not continue to support its growth, because as the internal carotid passes through a foramen in the skull, a little above the swelling, it could not dilate at that part to bring down any additional quantity of blood into the sac; so that its first effect was likely to be as great as any it could produce. But if the aneurism had been of the external carotid artery, owing to the number of communicating vessels, I should not have been equally sanguine in my expectation that the pulsation would have ceased, as I have known two instances, one of a wounded radial artery, and the other of aneurism of the anterior tibial, in which the tumour continued to grow by anastomosis, after the arteries had been tied above the swellings.

CASE *of a* FŒTUS *found in the* ABDOMEN *of a* BOY. *By* GEORGE WILLIAM YOUNG, *Esq.*

From the same.

JOHN HARE, the subject in whom this curious phænomenon occurred, was born on the 18th of May, 1807. At the time of birth he appeared to be a healthy, well formed child. He was, however, soon troubled with frequent vomiting; discharging large quantities of fluid, which was sometimes of a green, at others of a yellow colour. A peculiarity in the form of his abdomen did not long escape notice; a prominence, easily discernible, presenting itself at the upper part, a little to the left of the scrobiculus cordis. The evident increase of this tumour, and the periodical vomiting, induced his mother to consult me respecting him. I saw him first on the third of September, at which time he had the general appearance of a plump and healthy child. His mother informed me that he lived chiefly upon the breast, and that the milk was not returned excepting when the bilious vomiting took place; which recurred at intervals of a week or ten days. His stools were of a green colour; he appeared to suffer pain, and his sleep was much interrupted.

On examining the abdomen, I found a round smooth tumour, situated evidently within that cavity, at its fore and upper part, immediately below the margin of the chest; occupying a space, bounded by an imaginary perpendicular line drawn from the apex of the third false rib on the left side, and meeting a transverse line, passing just below the navel; so that it was placed in the epigastric and umbilical regions, but inclining to the left side. Its extent towards the right could not be ascertained, as the child always appeared to suffer when that part was pressed, and the tension of the abdomen in the act of crying, rendered examination more obscure. The tumour was somewhat moveable; it was tense; and afforded a distinct sense of fluctuation at its most prominent part, where it was covered by the left rectus abdominis.

From this examination it was evident that the disease was not seated in the spleen; nor did it appear probable that it was an affection of the liver. I was therefore led to conclude that there existed in this part an original imperfection; probably a cyst adherent to the mesentery or vertebræ, distended by a fluid, and perhaps containing hydatids. With this view of the case, I saw no prospect of affording relief, excepting as far as the stomach and bowels were concerned; and, as I could not encourage his mother to expect a cure, I soon lost sight of him. It was not until the 7th of January in the present year (1808) that she again brought the child to me. I now found him a mere skeleton, clothed in skin, with a face of age and anguish. The account which was given of the interval was interesting. It was stated, that the child daily became thinner, whilst the tumour in the abdomen rapidly increased, until at length he was nearly thirty-six inches in circumference. His sufferings kept pace with this augmentation in bulk. He seldom slept, but was almost continually crying or screaming. He took very little nourishment, the pain compelling him to quit the breast almost as soon as he had been applied to it, and he refused every other kind of food. The tumour, during its increase, continued to preserve its peculiar shape; and did not equably distend the abdomen. It formed a tense projection forwards; whilst the flanks and the hypogastric region were soft. In this respect a remarkable change took place on the 23d of December, which was succeeded by some interesting phænomena.

For seven days and nights prior to this event the sufferings of the child were almost incessant; his cries were interrupted only by exhaustion and fatigue; and his death was hourly expected. On the day above noted, when his mother went to change his cloth, as she thought for the last time, she was greatly surprised to find the belly no longer tense at the part opposite the tumour, but equally soft in every part. She also perceived a remarkable change in his form. The anterior prominence was lessened, and the sides of the abdomen now projected greatly between the lower ribs and the cristæ of the ilia. He became quiet, and apparently easy for two days and nights, voiding in this time vast quantities of urine. This discharge continued about a week, and was attended by a corresponding diminution of the abdomen.

This narrative warrants the conclusion that the tumour consisted principally of a fluid, contained within a distinct cyst; that this cyst was ruptured on the 23d of December; that its fluid contents escaped into the cavity of the peritoneum, and that the absorbents of this extensive membrane rapidly removed them.

The vomiting, which prior to this event had daily recurred, now wholly ceased; he became ravenous, and could with difficulty be kept from the breast; he regained strength, and improved in appearance. This favourable change was not of long duration. The cyst soon began to fill again; for, when I saw him on the 7th of January, fifteen days after its rupture, the mother assured me he had already considerably increased. At this time the circumference of the abdomen was found to be eighteen inches and a half. Fluctuation could be distinctly felt in a cyst, not firmly distended; and a hard tumour was discovered, apparently floating in it, which easily slipped from the grasp; and endeavours to fix it appeared to give pain to the child;

I had now an opportunity of seeing him occasionally until the period of his death. The abdomen gradually increased, and its augmentation depended on an accumulation within the tumour, as its peculiar form demonstrated to the eye and to the touch. The child again lost his rest and appetite. Emaciated as I found him, he still further declined in appearance, and the vomiting returned. In relation to this symptom it is proper to note an appearance, often observed before by the mother, and now distinctly perceivable: prior to ejection, a pouch appeared to fill at the scrobiculus cordis, and to be pressed by the cartilages of the ribs against the tumour upon which it rested; so that, by the state of this part the mother could foretel the approach of vomiting, by which it was always emptied.

The inspection of the body after death fully explained this circumstance. On the 25th of February last he died. About twelve hours after death I examined the body in the presence of my friend Dr. Birkbeck, whose zeal for such inquiries induced him to accompany me.

Inspection of the Body after Death.

The abdomen when measured was twenty-two inches and a half in circumference. When this cavity was exposed no fluid escaped; it was occupied chiefly by a large and nearly spherical tumour, which in parts was somewhat transparent and appeared distended by a fluid. Above it, in the right hypochondrium, was seen the liver, much diminished in size; the fundus of the gall bladder was turned forwards and inwards towards the linea alba. At the scrobiculus cordis, lying on the upper part of the tumour, was seen the pyloric extremity of the stomach; which fully explained the appearance observed to precede the act of vomiting during life. The pylorus itself was scarcely distinguishable, therefore no means existed which were fitted to prevent a constant and free communication betwixt the cavity of the elongated stomach and the duodenum. The duodenum descended obliquely along the right side and upper part of the tumour; and then took its usual course behind it. The cæcum was not materially altered in position; but the colon ascendens, together with the arch of the colon, passed transeversely over the tumour, somewhat below its middle, and was firmly adherent to it; the tumour being evidently placed between the lamina of the transverse meso-colon. The diaphanous omentum was stretched over the tumour, betwixt the great curvature of the stomach and the arch of the colon; and the omentum minus was put equally upon the stretch; the small intestines were thrust down into the pelvis and hypogastric region, where, during life, they had been distinctly felt. The dense inferior part of the tumour rested on the mesentery. Before removing any part I looked carefully for a cicatrix which might mark the part through which the fluid must have escaped at the time the occurrences denoting a rupture of the cyst took place; but in this I was unsuccessful. I found the cyst thin and transparent, where it was covered by the omentum; thick, dense, and perfectly opaque below the arch of the colon. After raising the stomach from its situation, the pancreas was seen stretched out upon the cyst, and its transparent duct appeared running along the fore and upper part towards its opening into the duodenum. It was remarkably elongated, measuring nine inches. The little pancreas was widely separated from the larger portion of gland; remaining close to the duodenum at the termination of the elongated pancreatic duct. So much were these glandular substances compressed between the cyst and the upper layer of the transverse meso-colon, that in a hasty view they might have been passed by unnoticed. The splenic branch of the vena portæ also took its course on the anterior surface of the cyst towards Glisson's capsule. This bundle of vessels answered the purpose of a firm ligament suspending the tumour. The posterior surface of the cyst rested chiefly upon the aorta, and was adherent to the left crus of the diaphragm. The cœliac artery, elongated, ran upwards and forwards to reach the superior part of the tumour, where its three branches were distributed in the usual manner. The superior mesenteric artery ran downwards towards the small intestines, closely adhering to the posterior part of the cyst; and behind it the duodenum crossed the spine as usual. The vena cava passed on the right side, unconnected with the tumour.

After having thus ascertained the relative situation of the tumour, and removed it from the body, I punctured it: seventy-eight ounces, or four pounds fourteen ounces of a limpid fluid escaped, having the colour of an infusion of green tea, with a very slight tinge of blood. The opening was now dilated to expose the fleshy mass which had been felt during life, and it may be easily conceived that we were greatly surprised on finding that this substance had unequivocally the shape and characters of a human fœtus.

External appearance of the Fœtus.

The surface of this singular monster was covered with a quantity of sebaceous matter, such in all respects as is often met with on the skin of infants recently born. When this was removed the creature appeared as rosy and as healthy as if it had been yet alive. Its short and stout limbs were plump and firm; they were almost fixed in a posture resembling that in which the fœtus in utero is usually found. Its spine was greatly curved and formed a considerable rotundity backwards. The upper extremities laid close on each side of the trunk: the lower, which were remarkably short in proportion to their bulk, were drawn upwards towards the anterior part of the body, leaving the nates and genitals exposed below.

At the upper part of the trunk, above and between the shoulders was situated a dark red fleshy mass in the place of head, of which there was not any other vestige. This substance, when fresh, was plump and soft; careful dissection proved it to be of a texture resembling the pia-mater. It was plentifully supplied with blood vessels of considerable magnitude, but in no part of it could be found any substance resembling brain, nor could any nervous filaments be discovered in it. Across and into this substance ran a slender white cord, which was continued to the containing cyst and there attached; it measured about two inches and a half in length. This proved to be nothing more than a slip of dura-mater. Another portion of this membrane was seen covering and adhering to part of the anterior surface of the mass of pia-mater. But the chief connection betwixt the containing cyst and the fœtus was found at the umbilicus; to this was fixed the apex of a fleshy cone, the basis of which was formed by the inferior portion of the cyst, immediately before that part of it to which the beginning of the jejunum was attached. The side of this conical substance was of a full red colour, smooth, plump, and to the feel possessed a soft fleshy firmness.

The diameter of the base of the cone measured one inch seven-tenths.

Its extremity at the umbilicus, half an inch.

Its side, one inch three-tenths.

A diagonal incision through its dense circular base gave vent to a quantity of black tenacious matter much resembling the meconeum of infants; this, it was now found had been contained in several convolutions of intestine, one of which adhering to the part divided, had unavoidably been wounded: thus it appeared, that this fleshy cone was an exomphalos; but at the same time it formed an important bond of union betwixt the fœtus and the containing child, as will be shown in the sequel.

At the basis of the substance which occupied the place where the head ought to have been, were two locks of fine long hair of a light brown colour; and beneath these upon the thorax were two eminences. The one on the right, was of a button-like shape, its surface flat, its edge circular and rounded. It consisted merely of common integuments padded with fat. The lesser eminence on the left, together with its stalk or peduncle, by which alone it was connected with the body of the fœtus, contained the rudiments of a bony substance and some dense cellular membrane covered with common integuments.

The breach was well formed, the clutch or separation between the nates distinctly defined, but there was no anus. The genitals had all the external characters of a male; a penis with a loose and rugous preputium; a glans penis denuded and most perfectly formed; with a distinct orifice to the urethra; this canal was not continued more than a line in the substance of the penis where it then terminated.

The scrotum was divided into two parts towards the anus, but had no other character of labia. There was however an appearance under the penis, seen only when it was raised, which at first sight rendered the intended sex doubtful. This was a smooth red surface, with the aperture of a small and very short canal at its upper part, which extended inwards not more than a line; this was probably a continuation of the urethra, as it began nearly opposite the termination of that portion of the canal within the penis.

The right lower extremity consisted of a very short thigh, a distinct knee, a very short leg, a well marked ancle, and a correctly formed foot. The back of this foot rested against the shoulder of the same side, whilst the sole turned directly forwards. The heel and outer edge of this foot, and the hollow of the sole had all the most natural appearance; but the toes exceeded the usual number; four very small and separate phalanges, furnished with nails, held the place of the little toe and its neighbour; next to these were two large indistinct toes, each furnished with a nail, and the great toe was split into two smaller well shaped toes with nails. If then we consider these nails as pointing out the number of toes, it must be said that to this foot belonged eight toes.

The left lower extremity was not equally well formed. A thigh, a knee, and a leg were easily distinguished, but the foot was greatly misshaped, in the manner of a club foot; the sole turned backwards and rested against the body and left shoulder;

the heel and outer edge of the foot were turned inwards. The toes differed materially from the ordinary arrangement; three little toes lay evenly next each other towards the outer edge of the foot; whilst the great toe projected considerably outwards, like a thumb separated from the fingers; betwixt these was a short misshaped projection with a nail upon it. On this foot then there were five nails.

The right superior extremity consisted of an arm, an elbow bent and pointed forwards, a fore-arm with the hand directed backwards and resting against the side. The fingers were not complete either in number or form. There was one finger very well shaped; it had a perfect nail, the only one on this hand; on each side of this there was an imperfect stump, evidently intended for fingers.

The left superior extremity consisted of an arm, an elbow marked by a slight bend and a deep dimple, a fore-arm, a well marked wrist and hand, to which there were but two fingers; these were large, straight, and parrallel; on each there was a distinct nail.

The posterior region of the body exhibited, throughout almost its whole extent, a very singular appearance. The common integuments terminated abruptly on each side, and thus formed the boundary of a dark red surface, broad at the shoulders, and tapering to a point towards the sacrum, above which it terminated. On the integuments around it were a number of fine short erect hairs, which were more numerous towards the pelvis. Along the middle of this space, in the direction of the spine, ran a line or raphe, from each side of which passed off transversely numerous filaments. Their course was not straight, but rather serpentine, and they sent to each other, in an oblique direction, slender filaments of a similar structure; they became gradually shorter towards the inferior pointed extremity of this part. On each side of this part the dark red denuded surface was rough; but the villi, which gave it this appearance, had not any uniform or regular arrangement. Betwixt this and the edge of the integuments there was a margin of perfectly smooth and polished membrane.

The peculiar structure of this part rendered it a subject of curious attention in the dissection of the fœtus. On examining the spine it was discovered that there were no processes to the vertebræ, no vertebral canal, no spinal marrow; that the substance in queston, plentifully supplied with blood vessels, lay on the posterior surfaces of the bodies of the vertebræ, thus occupying the place of the medulla spinalis.

From these circumstances it appears warrantable to conclude that it was intended to form the spinal marrow, and that it consists of the membranous and vascular materials which appertain to it.

Dissection of the Fœtus.

It now became a matter of more interesting inquiry to ascertain the structure of this singular production; to discover the organs it possessed; to determine what were its functions, and in what manner they were performed; to trace its mode of connection with the containing child, and thus to explain its nourishment and growth.

This required some deliberation and care; as much deviation from the ordinary structure and situation of parts might be expected; as all lay within a small compass, and at a great depth from the surface; owing to the bent position of the body, the fixed posture, the shortness and the great bulk of the limbs. Much of the aid which injections afford, was not in this case to be obtained. The discovery of so singular a phenomenon was of course not looked for; and the removal of the parts from the body took place under circumstances which rendered the preservation of them hopeless. Many vessels were therefore wounded, which might have afforded means of filling their branches with wax. The cyst itself had been slit open, and the basis of the exomphalos divided, in order to examine its contents.

The investigation was begun by a perpendicular incision through the parietes of the abdomen, on the left side of the navel; and another, at right angles with this, slit the umbilicus open. A membranous pouch was now exposed, which appeared to occupy the whole cavity of the abdomen; from this proceeded an intestine through the umbilicus; but nothing else could be seen at this confined incision; and it therefore became necessary to make an extensive exposure of the whole interior. It appeared that this would be most safely effected by extending the vertical section, begun at the abdomen, through the thorax, down the spine and through the pelvis; as the corresponding edges of each portion of the section could be easily discovered, and the course of divided vessels be traced without difficulty. The deficiency of vertebral canal and spinal marrow was now ascertained; the bodies of the vertebræ being the only parts of the spine which had been developed. The small cavity betwixt this and the anterior parietes of the body contained but few parts; and these did not bear any very close resemblance to the usual contents of the trunk. There was no diaphragmatic partition dividing this cavity into thorax and abdomen. There was no heart, no spleen, no liver, no urinary organs, nor any internal organs of generation. At its upper and posterior part, close to the vertebræ, lay a very vascular substance of a pale rose colour, which, from its texture and situation, might be considered as intended for the lungs.

The alimentary canal was the most perfectly formed of the internal organs; part indeed of the intestines, situated in the exomphalos, was in all respects naturally constructed. Its commencement occupied the inferior and anterior part of the body, and entirely filled the pelvis; it consisted of that pouch, which has been already noticed. The complete section of the body at once exposed its cavity, which was filled with a coagulum of florid blood. That portion of this pouch which occupied the pelvis gradually contracted towards the anus, where it terminated with an impervious point; so that there was not any outlet. Behind the upper part of the pubis the substance of the pouch was folded transversely, and formed a ridge, which projected considerably into the cavity. The extremities of this fold were gradually lost on each side in the substance of the pouch. Above this transverse partial septum the cavity was again expanded, especially at its posterior part, from whence commenced a spiral course of the intestinal tube, narrowing in capacity as it passed towards the navel; giving to this part the appearance of a turbinated shell; the basis of which was in the cavity of the body, and the apex passed out at the navel; there was, however, no modeolus around which this portion of the alimentary tube wound itself. A correct idea of the construction of this part may be formed, by considering it as a conical tube, coiled, from right to left, like a cork-screw or a well-staircase. It formed three complete turns; and, having passed out at the navel, terminated in a sudden enlargement of the intestinal tube, which was the commencement of the first and most considerable convolution of intestine. This passed along the side of the exomphalos to its basis, to both of which it adhered; it then bent its course backwards towards the body; this portion was unadhering; the concave edge gave attachment to the mesentery, in which the natural course of the blood vessels was readily seen. Having nearly reached the navel it again adhered; then suddenly was lessened in capacity, forming a small tube of dense structure, which terminated in a singular three-sided pyramidal body, the apex of which was free. This body was of a firm fleshy consistence; its basis was united to the dense portion of intestinal tube just noticed, with which its narrow cavity was continuous. To one of its sides another convolution of intestine was attached, of less capacity and extent than that first described. The aperture of communication between this intestine and the three-sided appendix was large enough to admit a probe. From this last mentioned knuckle of intestine, which was supported by a distinct portion of mesentery, the intestinal tube was continued behind the great convolution adhering to the basis of the exomphalos. The remainder of the intestinal tube took a tortuous course across the basis of the exomphalos, to which it was firmly fixed, and terminated in the straight gut. This capacious intestine closely adhered to the right side of the exomphalos, passing from its basis to the umbilicus, near which it terminated by an external opening. This intestine was split open, which showed numerous folds on its internal surface; the termination of the preceding intestine; and a small opening which lead to an external aperture, through which a probe passed without force. Here then the anus was found; this outlet of the intestinal canal was situated on the right side of the exomphalos, near the umbilicus.

It would be tedious to enter into a minute description of the bones of this fœtus; the following general remarks have therefore been deemed sufficient.

There was an irregular bony substance at the upper part of the trunk, which may be considered as intended for the basis of the cranium. The spine, of which notice has already been taken, consisted only of the bodies of the vertebræ, in which ossification had not been tardy. There were but few ribs, and these very short. The pelvis consisted of a sacrum and two ossa innominata. The ileum was ossified, but the pubis and ischium were almost wholly cartilaginous. Of the cylindrical bones the bodies were ossified; but their apophyses were cartilaginous. The carpus, the tarsus, and the phalanges were entirely cartilage. Some of the joints were well constructed; the extremities of the bónes which formed them were covered with diarthrodial cartilage; they were united by firm ligaments and lubricated by synovia.

Very little muscular substance was to be met with; there was not any on the posterior part of the trunk; the anterior parieties of the abdomen were composed solely of common integuments, adipose substance and peritoneum. About the hip joints there were some slender portions of muscle; but little, if any, was discoverable in the remainder of the limbs; they principally consisted of adipose substance.

One of the most singular circumstances in the structure of this creature was the total absence of brain, of spinal marrow, and of the nerves of sense and voluntary motion; but a distinct plexus of nerves was seen just within the umbilicus, about the commencement of the intestines, to which numerous branches were distributed.

The sanguiferous system was without a heart. It consisted of two main trunks. One, which was ramified at each extremity, sent numerous branches from the middle of the basis of the exomphalos, into its laminated substance; which extended far beyond the circular limits, defined by the attachment of the side of the cone. The peculiar structure in which they ramify formed a considerable portion of the cyst, as will be seen in the account given of this part. This trunk was then placed between the intestines to which it sent branches; entered the umbilicus at its inferior part; passed first under and then to the right of the turbinated portion of intestine, and, lastly, entered the lung. It here divided into several branches, which were distributed to the extremities, to the spine, to the pelvis, and to the mass of pia-mater which held the place of head.

The other great trunk was placed on the right side of the first in the lung; where it received branches from the pia-mater, from the spine, from the pelvis, and from the extremities. As it passed out at the umbilicus, it gradually separated from the first mentioned vessel, and took a direct course between the inner surface of the side of the exomphalos and the straight gut. Having reached the basis, it ran a little way along its edge, and then took an extensive course on the inner surface of the cyst towards the superior mesenteric vessels of the containing child, near which it terminated. The considerable length of this vessel, it was evident, had been occasioned by the gradual augmentation of the cyst; it was so choaked up with coagulated blood that quicksilver could not be made to run any distance in it, and though it could not be traced to the neighbourhood of the superior mesenteric vessels, yet the greatest care and perseverance could not discover its mode of termination.

From what has been already stated, it must appear that the containing cyst answered the purpose of a placenta to the fœtus, and it therefore becomes a point of some interest to inquire into its structure. The thickness of the cyst was various; it was thinnest at its fore and upper part, where it was covered by the omentum; and, when fully distended, it was at this part transparent. Here an appearance was met with which explained the escape and re-accumulation of the fluid contents noticed during life. A laceration was seen on the inner surface about half an inch in length, which lead to a separation, of the extent of three quarters of an inch, between the two layers of which this part of the cyst was composed. At the termination of this separation there was a small hole through the external coat. This appearance, it is probable, had been produced in the following manner:—The great accumulation of fluid occasioned the internal coat first to give way, and this small rupture was, by the same cause, gradually enlarged. The external coat opposite this part had now to sustain the whole increasing force of distention; till at length yielding at the small point noticed, it allowed the fluid to escape into the posterior cavity of the peritoneum, and thence under Glisson's capsule, into its anterior cavity. In the flaccid state of the cyst, produced by the partial evacuation of its fluid contents, its vessels would in part repair this breach, and the separated laminæ would again be brought into contact; so that a fresh accumulation of fluid would be prevented from escaping.

The thickest part of the cyst was its inferior portion; the middle of which formed the basis of the exomphalos. It received a peritoneal covering, between the arch of the colon and the mesentery, from the inferior lamina of the transverse mesocolon; the superior lamina of which was spread over the upper part.

The internal surface of the cyst was lined by a smooth delicate serous membrane, which was reflected over the side of the exomphalos, and terminated as abruptly at the navel of the fœtus as the peculiar structure of the funis appears to end at this part in a fœtus placed under ordinary circumstances. At some parts of the internal surface there was a scaly appearance, resembling, in some degree, deciduous cuticle.

The substance of this cyst consisted of several layers of considerable firmness; varying at different parts in thickness and apparently in number. Opposite, and to some extent around the exomphalos, these laminæ appeared much thicker, more numerous, and more distinct than elsewhere; this part could be shown to consist of eight laminæ; they appeared distinctly in a vertical section; and separated readily. This part of the cyst received a very considerable artery from the colica sinistra; this vessel sent branches round the basis of the exomphalos, whose subdivisions crossed it in all directions; but no mesenteric vein accompanied this artery.

It may here be useful to recapitulate as much of the foregoing narrative as relates to the business of circulation. It has been seen that the fœtus was furnished with two vessels; a short one obviously ramified at each extremity; the branches of one extremity meeting with numerous arterial branches of the containing cyst; the other, a long vessel, separated during the greater part of its course from the first, running along the inner surface of the cyst, and terminating abruptly at its posterior part near the superior mesenteric vessels of the containing child.

Here then arises a difficulty in the way of considering the cyst as answering the purpose of a placenta; for to establish this opinion it would be requisite that each vessel should ramify in the substance of the cyst; and that its branches should be so situated as to admit of communication with those of the other vessel. Although such an arrangement did not appear, it is however probable. The greatest caution and assiduity did not lead to the discovery of the manner in which the long trunk terminated; it could not be traced into any branch of the mesenteric vessels of the containing child; (yet had this been the mode of termination, the size of the vessel renders it probable that it might have been detected;) nor could any branches be shown to proceed from it. Its extremity, however, inclines towards the exomphalos, and is not more than an inch and a half from it; the substance of the cyst is here considerable, and of the same structure as that in which the other vessel ramifies, and of which it is a continuation; branches may, therefore, be sent into this part; but, as quicksilver could not be made to run into, nor the eye to trace such branches, I am not authorized to assert their existence. It is, however, clear, that this vessel conveyed the venous blood of the fœtus from its body to the cyst, and that the fœtus was supplied with arterial blood from the cyst by the short doubly ramified vessel: but whether this vessel received its blood from the ultimate ramifications of the long venous trunk of the fœtus, or from the great arterial branch of the colica sinistra, I am not able to determine.

Attempts were made to ascertain whether there existed any direct communication between the branches of the colica sinistra and the cystic branches of the short fœtal trunk; but the incision, which had been made through the cyst at the basis of the exomphalos, had divided the most considerable branches of both these vessels, and allowed the quicksilver to escape too rapidly to admit of pressure from a considerable column. The injection did, however, run into some minute branches without in any instance entering the other set of vessels.

I here close the description of this extraordinary case, which, in conjunction with an account of a similar occurrence published a few years since, in a Bulletin de L'Ecole de Medicine de Paris, will tend to affix some credibility to similar cases met with in authors; with regard to these, however, it is to be regretted that they do not contain that internal evidence of their truth which an anatomical account of the appearances displayed by dissection would afford.

REVIEW.

An Essay on the Effects of Carbonate, and other Preparations of Iron, upon Cancer: with an Inquiry into the Nature of that and other Diseases to which it bears a relation. By RICHARD CARMICHAEL, *Surgeon.* 8vo. pp. 495. *Dublin*, 1809.

THE first edition of this work was published a few years ago, in the form of a pamphlet, since which the author has been so successful in his treatment of cancers, (according to his own account) that he is now enabled to present the profession with histories of upwards of thirty cases cured by the remedy then recommended—various preparations of iron, particularly the oxy-phosphate of that metal. But we fear other practitioners have not been equally fortunate. Mr. Pearson, who has written on the subject of cancer since the publication of Mr. C.'s pamphlet, takes no notice whatever of it, and thinks we are yet ignorant of a remedy for this dreadful disease. In the European periodical publications which we have examined, there is not a solitary instance mentioned in which Mr. C.'s plan of treatment has been tried. In a case of decided cancer, in which we used the oxy-phosphate of iron, both internally and externally, no benefit whatever was experienced by the patient. But although feruginous applications may be incapable of relieving genuine cancer, they may perhaps be serviceable in those obstinate ulcers, particularly about the face, frequently termed cancerous.

The train of reasoning by which Mr. C. was led to try preparations of iron in cases of cancer, is extremely hypothetical. Like Dr. Adams, he conceives carcinoma to be a sort of animal fungus, endowed with distinct vitality. But these gentlemen do not agree as to the part which possesses this life, Dr.

A. supposing it to be the matter contained in the cells of the cancer, and Mr. C. the cartilago—ligamentous substance composing the cancerous mass and branching out from it in a radiated form. This parusitic fungus, the latter thinks, resembles the water-polyp in this respect, that if it be divided into a number of pieces, each division becomes a distinct and perfect animal. And hence, when an unsuccessful operation for the extirpation of a cancer is performed, the progress of the disease is always accelerated, because, instead of one cancer you now have several. But if this reasoning were correct, then the cancer ought to re-appear in the same, or at least, very near the same place where it formerly existed, which is not the case. For in some instances, when a cancerous glans penis has been removed, the member remains sound, and the cancer attacks the groin, though that part was to all appearance perfectly healthy when the operation was performed, nor could "any roots" be discovered in that neighbourhood or direction. Moreover, why does the disease so generally follow the course of the absorbents?

Mr. Carmichael having satisfied himself of the vitality of cancer, naturally began to inquire what poison would be most likely to destroy this animal. Now it is found that iron, though innoxious to red-blooded animals, is more deleterious to those having white blood than even arsenick; Mr. C. therefore, immediately had recourse to it, and the following cases will show how successfully.

"I have now to relate a case, in which a remedy different from any that I had hitherto tried, proved successful; the preparations already mentioned in a great measure failed, and the inveteracy of the disorder resisted every application, except that to which I allude, *Arseniate of Iron.* This addition of the arsenious acid is far from supporting my theory; I tried it because arsenic had been recommended by others, and its utility in the form I used was undoubted. Some future investigator may do more than I am able in reconciling this circumstance to my theory, or perhaps more satisfactorily make it a foundation for overturning my speculations.

"The patient was William Carr, aged 60; he had the entire under lip indurated, enlarged, and on its upper surface ulcerated, accompanied with lancinating pains, and every other characteristic of cancer. He was a pale emaciated wretch, and the greater part of his life an habitual dram drinker.—The disease had been very slow in its progress, and had commenced seven years before, according to his own account, like a common blister. Before his application to me he resorted to Jervis Street Hospital, where the late Mr. M'Evoy told him his only chance was to submit to an operation.

"On the 14th of April, 1807—oxy-phosphate of iron was applied to the ulcerated part of his lip, and he was ordered fifteen grains of the carbonate three times a day.—This treatment was persevered in for six weeks, during which time the appearance of the ulcerated part considerably amended; and in place of the dry crust with which it was concealed, it was found covered at each dressing with white tenacious matter, and from being totally insensible, strange as it may seem, had become extremely irritable.

"From the beginning of June till the middle of July, the ulcer was dressed with sub-oxy-phosphate of iron, a preparation I have since found of the greatest efficacy in this disorder, though it produced in this case but little benefit; my chief motive for making use of it was its solubility in animal fluids, though I acknowledge its agreement in other circumstances which shall be mentioned hereafter, with my speculations, was no small inducement; this preparation always excited severe pain for eight or ten minutes after its application, but under its use the scirrhous part of the lip increased in volume, though it diminished in hardness,—from the time he came under my care he had not felt any lancinating pains.

"On the 29th of July, finding the disease stationary, neither improving or growing worse, I resolved upon trying the arseniate of iron, which Mr. Accum, of London, had just prepared for me. About a scruple mixed with the white of an egg was applied to the ulcer, the pain which followed was trifling, but the entire scirrhous tumour became considerably swelled.

"On the 1st of August the surface of the scirrhous was of a brown colour, and insensible to any stimulus. I removed the slough with the knife, and renewed the application to the living part of the diseased mass.—On the following day the entire lip was much swelled, although the application only excited a sensation of heat, and did not produce a greater degree of smarting than the sub-oxy-phosphate of iron.—I remarked that the fine skin of the lip, a part of which remained, was not affected by the arseniate; though the adjoining cancerous substance was rendered of a brown colour, and was swelled like a moistened sponge. During the remainder of this month the arseniate of iron was generally applied every third day, and as much of the diseased substance occasionally removed, as had become insensible and of a brown colour; in consequence he lost the greater part of the lip which was engaged in the disease, so that he found a difficulty to retain his saliva, or any liquid in his mouth. Towards the end of the month the effects of the arsenious acid on the constitution became evident, by the occurrence of frequent chilliness, loss of appetite and general debility. I therefore thought it advisable to lay aside the arseniate, and adopt once more the oxy-phosphate of iron; and under its use his general health improved, more of the diseased mass came away, a good discharge succeeded, and about the 14th of September the sore entirely healed, without leaving any perceptible induration behind. He however did not long survive his deliverance from cancer, for in the latter end of the succeeding November, he was seized with typhus fever, for which he was admitted into the hospital of the House of Industry, where he died. After his death I took the opportunity of examining his body. There was no vestige of any remaining carcinomatous substance on his face—in the brain there was nothing remarkable, but the lungs were full of *tubercles* and *vomicæ*, the œsophagus through its whole length was thickened in a remarkable manner, and in the stomach near the cardiac orifice there was a tumour the size of a hazel nut; on dividing which it exhibited the same structure as other carcinomatous tumours."

"Diana Blackburne, aged 58, applied to me on the 6th of July, 1807, with an open cancer of the breast, the ulcer deep and irregular, with elevated edges, was about the size of a half crown piece, and situated in a schirrous mass, which engaged at least a third of the breast—part of the nipple had been destroyed by the disease, and the remainder, which was undermined by the ulceration, appeared as if ready to drop off.—The lancinating pains were frequent, but not very severe; she was of a healthy complexion and appearance, and had not in any degree the sallowness which in general accompanies cancer. She attributed the disease to a severe contusion which she received on her breast about eighteen months before, as she observed, about three months afterwards, on the spot where she was injured, a small lump *like a kernel*, which gradually increased to the size of the fist, and it became ulcerated eleven months after its first attack. She appeared of so strong a constitution, that I ordered her thirty grains of the carbonate of iron three times a day, which she took without any inconvenience, and I dressed the ulcer in the usual manner with the oxy-phosphate of iron.

"On the 12th of July she was entirely free from shooting pains, but no other alteration was observable till the 18th. Therefore, in place of carbonate she was ordered to take twenty grains of the oxy-phosphate three times a day.

"26th.—There was a considerable degree of soreness and inflammation of the entire breast, the integuments of which surrounding the ulcer had become of a bright inflammatory red colour; but notwithstanding the increased sensibility of the part there were not any shooting pains.

"29th.—She informed me that several large lumps, or substances like cores were discharged from the ulcer, one of which she said was hanging loose from the sore. The ulcer had become more extensive, and was covered with a white cohesive matter which could not be washed away.

"August 5.—The ulcer had a more healthy appearance, and its sides were closing towards each other. I wished to remove the nipple, which hung loosely over the excavation of the ulcer, connected to the breast only by a small slip of integument, and producing great irritation, but she would not allow me.—She informed me that many ropy substances, some above three inches long, had come away with the discharge.

"10th.—The sore which had closed to nearly one half of its former size, appeared much improved, and discharged thick purulent matter. The external application of the oxy-phosphate of iron was still continued, and the same preparation was taken internally in large doses as before ordered, and agreed perfectly well with her stomach.

"September 2d.—The breast seemed entirely free of induration, except at the superior part of the sore, where some hardness was perceptible, in which she felt occasionally slight stinging pains; to this I applied about ten grains of the arseniate of iron, which, on the 8th, when I again saw her, had caused the ulcer to enlarge, by producing a slough and a more considerable discharge.

"September 20th.—The ulcer was diminished so much that it might be covered by the point of the finger; its reduction seemed not to be so much owing to the formation of granulations, as to the closing of its sides towards each other, which gave the surrounding integuments a pursed appearance. The induration was much less perceptible on the superior part of the ulcer, and the pains had altogether ceased. The oxy-phosphate had been in perpetual contact with the ulcer since she came under my care, except on the 2d of September, when the arseniate of iron was applied—and she was now directed to persevere with the former of these preparations.

"The ulcer continued stationary until the middle of October, about which time it became extremely sore and irritable, with an increase of the discharge and an inflammatory redness of the surrounding integuments. Conceiving that this inflammation was owing to the stimulus of the dead carcinomatous substance on the sound parts, I directed the application of emollient poultices, with a view of assisting the separation of the slough. With these she persevered till the end of the month, during which time large white sloughs, similar to those already mentioned, were discharged. The ulcer, in consequence, became more open and extensive, but its edges were soft, and there was not the least induration discoverable in any part of the breast. About this period I sent her to Mr. Richards and Mr. Piele, with a request that they would examine her breast, as this case I thought as strongly evinced the efficacy of the preparations of iron as any which had yet occurred to me. After the separation of the sloughs the discharge became thick and purulent, the sore filled with granulations, and was completely healed in less than a fortnight. I saw her three months afterwards, when she told me she had not felt the slightest pain or induration, but continued perfectly well."

"Elizabeth Slaven, a healthy looking child, 12 years of age, was brought by her parents from the town of Kildare, for admission into the hospital of the House of Industry, in the beginning of March, 1808. There was an ulcer situated within the nostril, the extent of which it was impossible to discover, on account of the scabs which filled the nostril; but the greater part of the nose was swelled, of a dusky red colour, and painful to the touch. She had had this ulcer upwards of a year, accompanied with stinging pains. On being questioned, she said that prior to the ulcer she had been in the habit of constantly picking her nose.

The carbonate of iron was applied on her admission, and an immediate amendment took place, as the pain, redness, and swelling of her nose began to decline, and the ulcer mended so rapidly, that in four weeks the scabs were entirely removed, and the ulcer healed. She was discharged the hospital perfectly well on the 15th of April."

"Mrs. L——, residing in Charlotte Street, a lady of about 40 years of age, of a pale sallow complexion, applied to me in April, 1808, on account of a hard tumour in each breast, attended with severe shooting pains; the chief mass of the tumours was situated between the nipples and axillæ, and the hardness could be traced running along the lower edge of the pectoral muscles, even into the axillæ; but this was more evident on the right side, where it felt (if I may be allowed to make use of an odd comparison) like thongs of leather loosely platted. The arm of that side she told me was frequently swelled, attended with numbness of her fingers. She dated the origin of her complaint so far back as the year 1799, and ascribed it to the anguish of her mind on being suddenly reduced, with a young family, from affluence to a precarious mode of subsistence, in consequence of having lost her husband, who was killed by the rebels in the county of Wexford.—This lady was not the only person under my care, afflicted with this disease, who supposed it owing to a continued state of grief and despondency. It has been observed by more than one writer on the subject, and there can be scarcely a doubt of the fact.

"She was put upon a course of ferruginous preparations, though I own, on account of the occasional swelling of her arm and numbness of the hand, I had little hopes of their proving serviceable; but it was not long till I had the pleasure of relinquishing this opinion. She was ordered ten grains of the sub-oxy-phosphate of iron three times a day, and to apply to the breast a lotion composed of acetate of iron diluted with five or six times its weight of water.—After she commenced this plan the lancinating pains became gradually milder, and soon entirely ceased; three days after she began to take the medicine, she passed some large worms, a complaint of which she had not the slightest suspicion.

"She persevered with little alteration in the plan above mentioned for four months, with a surprising amendment, not only with respect to her local complaints, but as to her general health; the hardness in her breasts became far less evident, and appeared to be gradually decaying; the lancinating pains, which were formerly very distressing, did not occur after taking the medicine three weeks, nor has the swelling in her arm since occurred; her appetite, appearance, and general health are improved to a degree that neither she nor her friends ever expected, and in October, 1808, any remaining hardness in her breasts was scarcely discernible. She, however, still continued to persevere in the external and internal use of the ferruginous medicine."

Cancer is not the only disease which Mr. Carmichael thinks may be cured by iron. Consumption he thinks may be owing to an excess or deficiency of this metal in the blood, and is to be cured accordingly. The diagnam is sufficiently easy, and the practice sufficiently simple. Where consumption comes on in consequence of inflammatory affections of the chest, attended with considerable arterial action and florid colour of the countenance, then an excess of iron in the blood is the same, and the cure is to be attempted by restraining the patient to those things which contain least iron, such as vegetables, and more particularly white blooded animals. Tubercular consumption, attended with palid complexion, is to be cured by giving some preparation of iron.

Where this remedy produces a sort of fever, as it sometimes does, it is to be stopped, and, if necessary, camphor is to be ministered. Costiveness is to be obviated by a few grains of aloes.

Although Mr. Carmichael possesses much of the enthusiasm of a discoverer, he admits he has failed in some cases of cancer. As our readers will now be enabled to try Mr. C.'s plan, we sincerely hope they may meet with the same success, and find *only some* cases which are intractable. Should they be so fortunate, and will communicate their success, nothing would give us greater pleasure than to announce that this opprobrium of Surgery is at length removed.

CASES *of* ORGANIC DISEASES *of the* HEART. *With Dissections, and some Remarks intended to point out the distinctive Symptoms of these Diseases. Read before the Counsellors of the Massachusetts Medical Society.—By* JOHN C. WARREN, *M. D.*

THERE is perhaps no subject connected with the healing art which has been so little cultivated in this country as morbid anatomy. So inveterate are the prejudices which prevail here with regard to examinations after death, that an American physician rarely has it in his power to subjoin to the history of an unsuccessful case, an account of the appearances on dissection; and it is almost unprecedented for one to examine a sufficient number who have died of any particular complaint, so as to be enabled to give a general history of the disease, and the morbid alterations of structure by which it was produced, or which it may have caused. Contrary to what happens in other countries, this regard for the deceased, as it is termed, is here most prevalent among the richer and higher orders of society. Although many valuable lives are constantly sacrificing to this mistaken opinion, we have little hopes of seeing it speedily eradicated; for as long as mankind can disguise their failings under specious epithets, as long as they can flatter themselves that this highly culpable weakness is no more than a decent regard for a dead relative, they will be incorrigible. The publication of Dr. Warren's book induces us to hope that the citizens of Boston are less tainted with this prejudice than their more southern neighbours, and we are exceedingly glad to give every publicity and commendation in our power to so praiseworthy an example.

The first instance of the disease related by Dr. Warren, is that of the late Governor Sullivan, which we shall extract, although it is somewhat long, because it is one of the best narrated cases we have ever seen, and from the celebrity of the person who was the subject of it, we take the greater interest in the recital.

" The symptoms of organic disease of the heart are marked with extraordinary clearness in the following case. The opportunity for observing them was very favourable; and there was every incitement to close observation, which could arise from the important and interesting character of the patient. These advantages will justify an uncommon minuteness in the detail of the case; especially as the most accurate knowledge of a complaint is obtained from a successive view of its stages.

" The late Governor of this commonwealth was endowed with most vigorous powers of mind and body. At the age of sixteen he was attacked with fits of epilepsy, which first arose from a sudden fright received on waking from sleep in a field, and beholding a large snake erecting its head over him. As he advanced in life they became more frequent, and were excited by derangement of the functions of the stomach, often by affections of the mind, by dreams, and even by the sight of the reptile which first produced the convulsions.

" At the commencement of the American revolution he became deeply engaged in public affairs; and from that time devoted himself to intense application to business, with which the preservation of his health was never allowed to interfere. In the expedition against Rhode-Island, an attack of inflammation of the lungs had nearly proved fatal to him.

" In the beginning of the year 1807, he suffered severely from the epidemic catarrh; and a remarkable irregularity of the pulse was then perceived to be permanent, though there is some reason to believe, that this irregularity had previously existed, during the fits of epilepsy, and for a few days after them. *In the summer, while he was apparently in good health, the circulation in the right arm was suddenly and totally suspended; yet without loss of motion or sensation.* This affection lasted from noon till midnight, when it as suddenly ceased, and the circulation was restored. In the autumn he was again seized with the influenza, which continued about three weeks, leaving a troublesome cough of two or three months duration, and a slight occasional difficulty of breathing, which at that time was not thought worth attention. Soon after, in November, he had one or two singular attacks of catarrhal affection of the mucous membrane of the lungs, which commenced with a sense of suffocation, succeeded by cough and an expectoration of cream-coloured mucus, to the quantity of a quart in an hour, with coldness of the extremities, lividity of the countenance, and a death-like moisture over the whole body. These attacks lasted six or eight hours, were relieved by emetics, and disappeared, without leaving a trace behind.

"At this time he began to complain of palpitations of the heart; yet, it is probable, that he had been affected with these before, since he was unaccustomed to mention any complaint which was not sufficiently distressing to require relief. He experienced a difficulty of respiring, as he ascended the stairs, and became remarkably susceptible of colds from slight changes of clothing, moisture of the feet, or a current of cold air. His sleep was unquiet in the night, and attended with very profuse perspiration; and in the latter part of the day, a troublesome heaviness occurred. The sanguiferous vessels underwent an extraordinary increase, or, at least, became remarkably evident. The pulsation of the carotid arteries was uncommonly strong; the radial arteries seemed ready to burst from their sheaths; the veins, especially the jugulars, in which there was often a pulsatory motion, were every where turgid with blood. The countenance was high coloured, and commonly exhibited the appearance of great health; but, when he was indisposed from catarrh, this florid red changed to a livid colour; which also, after an attack of epilepsy, was observable for two or three days on the face and hands. This lived hue was often attended, under the latter circumstances, with something like ecchymosis over the face, at first formidable in its aspect, and gradually subsiding, till it had the general appearance of an eruption, which also soon vanished.

"These symptoms increased almost imperceptibly, during the five first months of the year 1808. Much of this time was passed in close application to official duties; and it seemed that a constant and regular occupation of the mind had the effect of obviating the occurrence of any paroxysm of disease, as well of epilepsy, as of difficult respiration; and that a very sudden and disagreeable impression generally produced either one or the other. There were, indeed, independently of such circumstances, some occasional aggravations of those symptoms. Some nights, for example, were passed in sitting up in bed, under a fit of asthma, as it was called; sometimes the mind became uncommonly impatient and irritable; the body gradually emaciated; yet the appetite and digestive functions remained principally unimpaired; and persons around were not sensible of any material alteration in the condition of the patient.

"On the approach of warm weather, in June, the violence of the symptoms increased. Paroxysms of dyspnœa occurred more frequently, and were more distressing. They commenced with symptoms of slight febrile affection, such as hot skin, hard, frequent, and more irregular pulse, disordered tongue, loss of appetite, and derangement of the digestive functions. This kind of paroxysm lasted two or three days. Evacuations of blood from the nose and hæmorrhoidal vessels, which before rarely occurred, became frequent; a fulness at the upper and right side of the abdomen was sometimes perceptible, formed apparently by temporary enlargement of the liver; the difficulty in ascending an eminence increased sensibly. In the intervals of these attacks, which were variable, but generally continuing ten or twelve days, the strength was frequently good, and accompanied by a great flow of spirits, and an aptitude, or rather ardour, for business.

"Such was the course of this complaint until the latter part of August, when a very severe paroxysm occurred. It commenced, like the former, with febrile symptoms, but those more violent than before. The countenance became high coloured; the dyspnœa excessive, and rendered almost suffocating by a slight movement, or attempt to speak; the pulse hard, very irregular, intermittent and vibrating; and the digestive functions were suspended. These symptoms soon increased to the highest degree. The respiration was so distressing, as to produce a wish for speedy death; the eyes became wild and staring. No sleep could be obtained; for, after dosing a short time, he started up in violent agitation, with the idea of having suffered a convulsion. During the few moments of forgetfulness, the respiration was sometimes quick and irregular, sometimes slow, and frequently suspended for the space of twenty-five, and even so long as fifty seconds. At the end of three days the febrile heat was less permanent; the red colour of the face changed to a death-like purple; the hands and face were cold, and covered with an adhesive moisture; the hardness of the pulse diminished, and a degree of insensibility took place. I seized this opportunity to examine the region of the heart, which had not been done before, from fear of alarming the active and irritable mind of the patient. The heart was perceived palpitating, obscurely, about the 7th and 8th ribs; its movements were very irregular, and consisted in one full stroke, followed by two or three indistinct strokes, and sometimes by an intermission, corresponding with the pulse at each wrist. The pulsation was felt more distinctly in the epigastric region. During this paroxysm a recumbent posture was very uneasy, and the patient uniformly preferred sitting in a chair. When the recumbent posture was assumed, the head was much raised, inclined to the right side, and supported by the hand; the knees were drawn up as much as possible. He could not bear an horizontal posture; nor did he ever lie on the left side, except a short time after the application of a blister. At the end of the fifth day his sufferings abated, but the sudden affusion of a small portion of a cold liquid on the head produced a severe fit of epilepsy. This was followed by a return of the symptoms equally distressing, and more durable, than in the first attack.*

"This violent agitation gradually subsided, and was followed by a pleasant calm. The natural functions resumed their ordinary course; his appetite returned; his enjoyment of social intercourse was unusually great; and he amused and instructed his friends by the immense treasures of information which his talents and observations had afforded him, and which, he seemed to feel, would soon be lost. At the end of September the feet began to swell, and after some time the enlargement extended up to the legs and thighs, and increased to an extraordinary degree; the abdomen next swelled, and, after it, the face. Toward the end of October there were some indications of water in the chest; there was a constant shortness and difficulty of breathing; the cough, till now rare, became more frequent and troublesome; the contraction of the thoracic cavity rendered the action of the heart more painful, so that beside an uniform stricture across the breast, he sometimes described a dreadful sensation like twisting of the organs in the thorax. He suspected the existence of water there, and was inclined to consider it as his primary disease, but was easily convinced of the contrary. At one time he had a suspicion of a complaint of the heart, and, although he had never heard of a disease of that organ, [he] slightly intimated it to one of his friends, and mentioned a sensation he had experienced in the chest, which he compared to a fluid driven through an orifice too narrow for it to pass freely. In this month, beside the dropsical affections and increase of cough, he had occasional painful enlargements of the liver, frequent starting up from sleep, a slight degree of dizziness, a great disposition for reveries, and sometimes extraordinary illusions, one of which was, that he was two individuals, each of whom was dying of a different disease. This idea often occured, and gave him much uneasiness. He was also afflicted with long continued frightful dreams, and sometimes a slight delirium.

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* "During this time it was thought advisable to acquaint his friends, that an organic disease of the heart existed, which doubtless consisted in an ossification of the semilunar valves of the aorta, attended, perhaps, by enlargement of the heart; that the disease was beyond the reach of art, and would prove fatal within three months, possibly very soon; that if it lasted so long, it would be attended by frequent recurrences of those distressing symptoms, general dropsical affections, and an impaired state of the mental faculties."

" After the use of much medicine, on the 6th of November the effused fluids began to be absorbed, and passed out through the urinary organs with such rapidity, that on the 12th the dropsical enlargements had nearly disappeared. The pulse was much reduced, in hardness and frequency, by the medicine, and, as it fell, he became more easy. On the 10th the state legislature convened, and the call of business roused, like magic, the vigour of his mind; and the symptoms of his disease almost disappeared. During this session he made little complaint, dictated many important communications, and attended to all the duties of his office, without neglecting the most minute. As soon as the legislature adjourned, he declared, that his work was finished, and that he had no desire to remain longer in this world. He entreated that no farther means should be used to prolong his existence, and immediately yielded himself to the grasp of disease, which appeared waiting with impatience to inflict its agonies.

" From this moment the distressing difficulty of breathing had very slight remissions. The consequent disposition to incline the superior part of the body forward, for the purpose of facilitating respiration, increased so much, that he frequently slept with his head reposed on his knees. The cough became occasionally very violent, and was always attended with an expectoration of a brown coloured mucus, sometimes tinged with blood. The abdominal viscera lost their activity. The face was sometimes turgid and high coloured, at other times pallid and contracted. A gradual abolition of the powers of the mind ensued, with a low delirium, and two short fits of phrenzy. The state of the circulation was very variable; the pulse at the wrists principally hard and vibrating, rarely soft and compressible; the less pulsations becoming more indistinct, and at length scarcely perceptible. No perfectly distinct beat of the heart was felt, but a quick undulating motion, not corresponding with the pulse at the wrist. Three days before death the arteries assumed this undulatory motion, corresponded with the motion of the heart, and, for forty-eight hours, lost the irregularity of pulsation.*

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* " The celebrated Morgagni has recorded some cases of organic disease of the heart, discovered by dissection, the symptoms of which do not exactly accord with

"Once or twice the expiring faculties brightened. On the 30th November he awoke, as if from death, conversed very pleasantly for two or three hours, and humorously described scenes which he had witnessed in his youth.

"On the 4th of December came on the second attack of furious delirium. Insensibility and great prostration of strength ensued. The respiration became very slow, and obstructed by the accumulation of mucus in the lungs; the pulse very intermittent, then regular, and finally fluctuating. A hiccough commenced; coldness of the extremities and lividity of the face followed, and continued three days before death. On the 9th the incurvated posture was relinquished, and the head sunk back upon the pillow; the respirations then diminished in frequency, till they became only two in a minute; and at the end of twenty-four hours they very gradually ceased.*

DISSECTION NINE HOURS AFTER DEATH.

EXTERNAL APPEARANCE.

"The whole body was much emaciated; the face pale and contracted. The hands were slightly œdematous. Discolourations, answering to the ribs, were observed on the thorax; many small purple spots, hard and prominent, on the back; excoriations on the nates; and purple spots, resembling incipient mortification, on the heel and toe.

THORAX.

"The integuments of the thorax were free from fat: the cartilages of the ribs ossified in various degrees, some perfectly, others slightly. Upon laying open the cavity of the thorax, it was found to contain about three pints of water, the proportion being greatest on the left side.

"The lungs were contracted into a smaller compass than usual, and were very firm to the touch. Their colour anteriorly was whitish, with small distinct purple spots; posteriorly, of a deep red, with similar spots. The right lobe adhered

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those observed in this and the succeeding cases. It should be remembered, however, that many of the subjects of those cases were not examined by him while living, and others but a very short time before death. But it appears, that, in the last stage of this disorder, some of the most important symptoms may be materially changed, especially the state of the pulse, dyspnœa and palpitations. Thus in the case related above, and in some others, the pulse became regular, the palpitations subsided, and the dyspnœa was less observable. The cases of that accurate anatomist, therefore, are not so contradictory of those related here, as might at first be imagined."

* "Governor Sullivan was born December 4th, 1744, and died December 10th, 1808."

closely to the pericardium; it also adhered to the pleura costalis, by a great number of strong cords, which seemed to be elongations of the original adhesions. Some of them were nearly as hard as ligament, and many an inch in length. Internally the lungs presented a very compact structure. Their cells were crowded with mucus, and their vessels filled with black blood, partly fluid, and partly coagulated. Some portions were firmer and more condensed than others, but no tubercles were discovered.

"The pericardium, viewed externally, appeared very large, and occupied almost the whole space behind the opening formed by removing the sternum and cartilages of the ribs. It was situated principally on the left side, and contained about double the usual quantity of water; but was principally filled by the enlarged heart, to which it adhered anteriorly about two inches, near its base. Its parietes were, in every part, very much thickened and hardened.

"The heart presented nearly its usual colour and form, excepting on its anterior surface, which was somewhat discoloured by coagulated lymph. It was enlarged in bulk to at least one half more than the healthy size. The auricles and ventricles contained coagulated blood. The tricuspid valves were in a sound state. The left auricle was double the usual size. The left ventricle was enlarged, about three times thicker and much firmer than usual. The mitral valves were very much thickened, and near the insertion of their columnæ, which were sound, cartilaginous, so that they were quite rigid, and the opening made by them, from the auricle to the ventricle, was scarcely large enough to admit the passage of a finger. The semilunar valves of the aorta were ossified at their bases and apices, and the portion intermediate, between the base and apex, partly ossified, and partly cartilaginous, so as to render the valves very rigid. The aorta was at least one half larger than usual, especially at its arch. The arteria innominata, the carotid, and subclavian arteries, were uncommonly large and thick. The coronary arteries were considerably ossified.

ABDOMEN.

"The omentum was destitute of fat. The stomach distended with flatus on the pyloric side; its cardiac extremity, lying under the liver, was pressed down and contracted. The liver was shrunk; its tunic corrugated, as if it had been distended, and bearing marks of inflammation; its substance harder than usual; its vessels, when divided, pouring out liquid black blood. The gall bladder was filled with bile. The kidneys were thicker and more irregular in form, than is common. The abdominal cavity contained some water.

HEAD.

"The bones of the cranium were unusually thick. The dura mater, which was thickened, and in many places bore marks of former inflammation, adhered to the bone at the vertex. On its internal surface, near the longitudinal sinus, there was a small ossified portion, half an inch long and the eighth of an inch thick. The convolutions of the brain were narrow, and very strongly marked. The pia mater bore marks of pretty extensive inflammation, and adhered to the dura mater at the vertex. The cortical substance ran deep into the medullary part of the brain. The ventricles contained about double the usual quantity of water; their parts were all remarkably well defined. The vessels of the pia mater, over the corpora striata, were unusually injected with blood. The velum interpositum was very firm; the plexus choroides uncommonly thick, but pale; the opening from the right to the left ventricle large. The vessels of the brain were generally not much filled with blood.

"The blood appeared every where fluid, except in some portions of the lungs, and in the cavities of the heart. It was very dark coloured, perhaps more than ordinarily thin, and oozed from every part which was cut.

"The cellular membrane, in all dependent parts, effused, when cut, a serous fluid."

The attention of Dr. Warren, like that of almost every author who writes upon an individual subject, is so entirely engrossed by this one, that he can attend to nothing unconnected with it, otherwise he never could have passed over without observation so extraordinary a circumstance as that of the "*circulation being suddenly and totally suspended in the right arm, without loss of sense or motion, from noon to midnight.*" We confess we are totally at a loss to conceive how such an effect could take place. That the circulation may be suddenly and considerably diminished there is no doubt, but how it can be totally suspended in a particular part only, and that too without loss of sense or motion, we cannot imagine.

The Doctor seems, in some instances, to attribute too much to the disease of the heart, and too little to the affections of the other viscera. The following is the dissection of the second case.

DISSECTION TWENTY-FOUR HOURS AFTER DEATH.

"On opening the thorax, its right cavity was found to contain a large quantity of water; the left, a smaller quantity.

"The lungs were of a firm, condensed texture, especially at the lower part, where their solidity was nearly equal to that of a healthy liver. They contained black blood.

"The heart was much enlarged, and proportionally thickened. Its tunic was in some places covered with coagulated lymph, especially over the coronary arteries. Its cavities were filled with black coagulum, which in the right auricle and ventricle had a slight appearance of polypus. The semilunar valves of the pulmonary artery and aorta were unusually small, and their bases cartilaginous. Those of the aorta had lost their form, and were slightly ossified. The remaining valves were partially thickened. The arch of the aorta was very much dilated, its external coat covered with a bony crust, which extended through the remaining thoracic portion, gradually diminishing. This portion was also considerably dilated.

"The liver was indurated; its peritonæal coat exhibited a flaccid or wrinkled appearance, and bore marks of slight inflammation. The gall bladder was filled with bile, and the pancreas indurated."

Now, from the above account it appears to us that the diseased state of the lungs would much more naturally account for the death of the patient than the enlargement of the heart, which we rather think was a consequence of the former, arising from the greater quantity of blood necessarily thrown upon the heart from the difficulty of its transmission through the lungs, whereas such an enlargement of the heart would scarcely have produced the latter effect. We are the more anxious with regard to this, because this affection of the lungs is probably more under our control, particularly in its first stage, than an enlargement of the heart. The remedies for both are the same, depletion, digitalis, and the antiphlogistic treatment generally.

From the copious extracts which we have given from Dr. Warren's work, our readers will be able to judge of its merits; we shall not, therefore, obtrude our opinion upon them. We cannot, however, take leave of it without adverting to the printing, which is really an honour to the state of the arts in our country. The plates are also extremely neat.

The PHYSICIAN'S VADE MECUM*: Containing the Symptoms, Causes, Diagnosis, Prognosis, and Treatment of Diseases. Accompanied by a select Collection of Formulæ, and a Glossary of Terms. By* ROBERT HOOPER, *M. D. Licentiate in Physic of the University of Oxford, and the Royal College of Physicians of London; Physician to the St. Mary-le-Bone Infirmary; and Lecturer on Medicine in London. From the London Copy. This Philadelphia Edition is improved by a Translation of all the Latin Prescriptions, and is enlarged by an alphabetical List of all the Medicines now in use, with their Names in both Latin and English: And by other valuable Addenda.* 12mo. pp. 347. *Philadelphia.* Humphreys. 1809.

Same Work, "with a Translation of the Formulæ, and Additions and Alterations, adapted to the American Climate, &c. By a Practitioner of the State of New-York. 12mo. pp. 276. *Albany.* Backus. 1809.

THIS work, the author tells us in the preface, was "committed to the press, with the hope of its proving useful to Students, and those Practitioners in medicine who, from their professional occupations or other circumstances, may not have it in their power to consult the more voluminous works that have contributed so much to the improvement of medicine." Now, in our opinion these are the two very classes of persons to whom the work will not only be of no use, but injurious, if they place any reliance on it. For the directions which it contains are always so concise, frequently so vague, and sometimes so erroneous, that they are better calculated to mislead than to inform such readers. If there be any members of the profession to whom such a compendium can be useful, it must be to those practitioners only who have advantageously studied the voluminous authors above mentioned, but who may occasionally wish to consult a book of easy reference, containing at the same time a collection of useful formulæ.

To the correctness of the above opinion we think our readers will accede, when they have read the following plan of treatment recommended by Dr. H. for the cure of Intermittent Fever.

"TREATMENT.

"*In the paroxysm.*

"*Indications.*
1. During the cold stage—to endeavour to induce the hot.
2. During the hot stage—to promote perspiration.

"1. By artificial warmth; the pediluvium; fomentations to the feet; the warm bath; warm diluent liquids; cordial diaphoretics; and opiates.

"R. Aquæ ammoniæ acetatæ *dr.* ii. j.
——— cinnamomi,
——— distillatæ, *aa dr.* v.
Syrupi corticis aurantii *dr.* j.
"1. Misce ut fiat haustus quavis hora sumendus.

"TRANS.

"Mix into a draught to be taken every hour.

"R. Aquæ ammoniæ acetatæ *dr.* ij.
Misturæ camphoræ *dr.* x.
Vini antimonii gutt. viij.
Syrupi simplicis *dr.* j.
"M. f. haustus quavis hora sumendus.
"Mix into a draught to be taken every hour.

"HAUSTUS SALINUS COMMUNIS.

"R. Kali preparati *scr.* j.
Succi limonis *oz.* ss. vel q. s. ad ejus saturationem.
Aquæ cinnamomi *dr.* ij.

Aquæ distillatæ *dr.* viij.
Syr. corticis aurantii *dr.* j.
" M. pro haustu.
" Mix for a draught.
" This may be taken every half hour, or every hour.

" R. Spiritus ætheris nitrosi *dr.* ss.
Misturæ camphoræ *dr.* xij.
Aquæ ammoniæ acetatæ *dr.* ij.
Syrupi rosæ *dr.* j.
" M. f. haustus quavis hora sumendus.
" Mix into a draught to be taken every hour.

" R. Spiritus ætheris vitriolici compositi *dr.* ss.
Misturæ camphoræ *dr.* xij.
Syrupi simplicis *dr.* j. M.
" F. haustus quavis hora sumendus.
" Make a draught to be taken every hour.

" R. Pulveris antimonialis *gr.* vj.
——— opiati *scr.* ss.
Sacchari purificati *scr.* j.
" M. et divide in partes quatuor, quarum capiat unam quavis hora.
" Mix and divide into four parts, of which one is to be taken every hour.

" R. Pulveris ipecacuanhæ compositi *gr.* x.
——— tragacanthæ compositi *scr.* ij.
" M. et divide in partes quatuor æquales, quarum sumat unam quavis hora.
" Mix and divide into four equal parts, of which one is to be taken every hour.

" 2. By the use of cold acidulated liquids, and continuing the remedies recommended during the cold stage.

" If coma supervene—
" Adplicetur emplastrum cantharides capiti raso.
" Let a blistering plaster of cantharidis be applied to the head after it has been shaved.

" Emplastrum cantharides inter scapulas applicandum.
" A blistering plaster of cantharidis to be applied between the shoulders.

“ Admoveantur cataplasmata sinapeos pedibus.
“ Sinapisms (or mustard cataplasms) to be applied to the feet.

“ Should there be a congestion of blood in the external vessels of the head, or delirium, cupping from the temples, or leeches are to be applied, and opiates laid aside.

“ If there be any inflammatory diathesis, nitre is to be added to the medicines; and if any inflammation exist, it must be treated in the usual way. Vide *Phlegmasiæ.*

“ *In the intermission.*

“ *Indications.*

1. To excite a new action in the system by certain remedies, administered at the commencement, or immediately before the accession of the cold fit; and thereby to destroy the morbid concatenation induced by the cause of the disease.
2. To prevent the return of the paroxysms by invigorating the body.

“ 1. By emetics, by æther, or by opium.

“ PULVIS EMETICUS COMMUNIS.

“ R. Pulveris ipecacuanhæ *gr.* xv.
Ansimonii tartarisati *gr.* j.
“ Misce ut fiant pulvis.
“ Mix into a powder.

“ HAUSTUS EMETICUS COMMUNIS.

“ R. Pulveris ipecacuanhæ *gr.* xv.
Antimonii tartarisati *gr.* j.
Aq. Menthæ sativæ *dr.* xij.
Syr. croci *dr.* j.
“ M. pro haustu.
“ Mix for a draught.

“ R. Misturæ camphoræ *dr.* xiij.
Tinctura opii gutt. L.
Syrupi simplicis *dr.* j. M.
“ F. haustus.
“ Make a draught.

“ R. Ætheris vitriolici *dr.* j.

“ The patient is to take this undiluted: it deprives him of his breath for a short time, and excites great distress.

"2. By nutritive diet: regular exercise, if the state of the patient render its use practicable; and one of the following tonics:

"1. CINCHONA OFFICINALIS:—*The Peruvian Bark.* This heroic medicine is to be given in substance, and in large doses; if the interval be long, its exhibition should be delayed till within six or eight hours of the time of the accession of the cold fit; should there be any inflammatory diathesis, this ought first to be reduced during the hot stage of the preceding paroxysms, by saline diaphoretics; where there is great debility, the bark may be joined with wine and aromatics; if it occasion purging, with opiates and astringents; if costiveness, with rhubarb.

Here follow various tonics which it is unnecessary to repeat, and he concludes with the following.

9. "KALI ARSENICATUM:—This forms the basis of the tasteless ague-drop. It cures the most obstinate intermittents, but must be used with the greatest circumspection.

"SOLUTIO ARSENICALIS DI. FOWLER.

"R. Arsenici albi,
Kali preparati, *aa gr.* lxiv.
Aquæ distillatæ *oz.* viij.

"Boil until the arsenic is dissolved in a Florence flask placed in a sand bath, then add as much compound tincture of lavender as will increase the solution to one pound.

"R. Solutionis arsenicalis gutt. vj.
Aq. cinnamomi *dr.* xij.
Tincturæ cardamomi,
Syr. zingiberis, *aa dr.* j.

"M. f. haustus quarta vel sexta quaque hora sumendus.

"Mix into a draught to be taken every fourth or sixth hour.

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11. "HYDRARGYRUS:—Mercury is always necessary in the cure of intermittents when there is any visceral obstruction: a small dose should be given every night so as just to affect the mouth, and the tonic medicines are to be continued."

It is unnecessary for us to comment upon the inadequacy of most of the doses recommended by Dr. Hooper. Has he never had occasion to use cathartics in the cure of intermittent fevers?

The following are *all* his observations upon remittent fevers under that head.

"REMITTENT FEVER.

"A fever arising from the same *causes* as the intermittent; but in which, although evident and distinct exacerbations and remissions can be perceived, there is no complete interval or apyrexia; one exacerbation appearing not entirely to go off before a fresh attack ensues.

"The *symptoms* vary according to the situation and constitution of the patient, and the season of the year. Sometimes they are those pointing out a redundancy of bile; sometimes the nervous are most prevalent; at others the putrid.

"The protraction of the exacerbations generally arises from some cause which keeps up an irritation in the system, and thereby prevents the disease assuming its regular form; or it depends upon fever of another type having been accidentally superinduced.

"The *prognosis* will be drawn from the presence, or absence, of those circumstances which indicate danger in that particular form of fever which the disease assumes; and which are pointed out under the heads of the different species of typhus, synocha, or synochus. In warm climates it is often fatal.

"TREATMENT.

"The *treatment* will entirely depend upon the concomitant fever, or other cause which prevents the state of apyrexia, and gives to the disease the remitting form. Should it have a tendency to either of the following genera, the treatment will be such as is there laid down; if it depend upon some cause of irritation, as diseased viscera, this is to be removed by the appropriate means elsewhere enumerated."

The following curious oversight occurs in the translation of one of the prescriptions in the Philadelphia edition, p. 29. "M. f. Enema purgans—*Make a purging powder.*"

The *valuable addenda* of the Philadelphia Editor consist of some common-place observations upon cow-pock; an account of galvanism, extending to almost twelve lines; "a table of apothecaries weights and measures; some of the abbreviations used in prescriptions; and an alphabetical list of all the medicines now in use, with their virtues and doses." The two last articles in this long catalogue we have been unable to find.

In the Albany edition the whole prescription is translated, which is certainly much better than the uncouth mixture of Latin and English exhibited in the Philadelphia edition.

Observations on the Epidemical Diseases of Minorca, from the year 1744 *to* 1749. *To which is prefixed, a short Account of the Climate, Productions, Inhabitants, and endemial Distempers of Minorca. By* GEORGE CLEGHORN, M. D. *Professor of Anatomy in the University of Dublin; with Notes by* BENJAMIN RUSH, M. D. *Professor of the Institutes and Practice of Medicine in the University of Pennsylvania.* 12mo. pp. 184. *Philadelphia.* F. Nichols. 1809.

The Works of Thomas Sydenham, M. D. on Acute and Chronic Diseases; with their Histories and Modes of Cure. With Notes, intended to accommodate them to the present State of Medicine, and to the Climate and Diseases of the United States, by BENJAMIM RUSH, M. D. *Professor of the Institutes and Practice of Medicine, and of Clinical Practice, in the University of Pennsylvania.* 8vo. pp. 473. *Philadelphia.* Benjamin & Thomas Kite. 1809.

THE works of these authors have acquired such celebrity, that any eulogy from us would be more than useless. In editing books of such acknowledged merit, Dr. Rush has done a service to the profession and to his country, by adding to them the popularity of his name to extend their circulation. To both he has subjoined pertinent notes, particularly to the latter; and to both he has prefixed a dedication to the students of Medicine in the University of Pennsylvania. In the dedication of Sydenham he has given a critique upon the work, and upon the notes which are added. We shall therefore extract it.

"The works of Dr. Sydenham are singular, in being alike celebrated and neglected by modern physicians. They owe their fame to the many invaluable truths that are contained in them; and the neglect with which they have been treated to certain errors, which have been refuted by modern discoveries and improvements in medicine.

"The design of this edition of the Doctor's works, is to revive and enforce those truths, and to enumerate those errors, so as to guard against their influence in practice. It is true, this has been done by Dr. Wallis and Dr. Swan, in their learned and elaborate notes; but those notes are adapted to the climate and modes of practice of Great-Britain and other European countries. They do not apply to the diseases of our country, nor to the practice which is accommodated to them; on the contrary, they are calculated to lessen the confidence of the reader in some of those prescriptions of our author that are suited to our diseases, and which derive their peculiar character from the sameness of the diet and manners of the inhabitants of Great-Britain about the middle of the 17th century, and of the present inhabitants of the United States. They are moreover composed of such numerous quotations from medical books, as to render them a system of the practice of physic, rather than short and practical illustrations of the truths and errors of the author.

"The principal errors in this work appear to be the following:

"1. A general belief in the salutary operations of nature in diseases.

"2 A belief in morbific matter as the cause of diseases, and in the hypothetical doctrines of 'fermentation,' 'ebullition,' 'digestion,' 'separation,' 'despumation,' and 'expulsion,' of this morbific matter, in order to cure those diseases.

"3. A belief in the practicability of dividing and distinguishing diseases by certain marks, such as characterize plants and animals, and which have since been attempted by Vogel, Segar, Linnæus, Sauvage, and Cullen. Happily for our science, the Doctor has furnished us, in his history of the combinations of epidemics, and of the changes of diseases into each other, with a complete refutation of this error.

"4. I consider our author's definition of a disease to be erroneous, viz. that it is 'a vigorous effort of nature to throw off morbific matter, and thus to recover the patient;' instead of which I believe a disease, to use the definition he has rejected, to consist 'in the confused and irregular operations of disordered and debilitated nature.'

"5. The preference which our author has given of vegetable to mineral remedies, is an error which has been refuted by the immense aids which medicine has lately derived from the mineral kingdom.

"6. His belief in a specific remedy for the gout, is an error which has tended very much to restrain free inquiry into the nature and cure of that disease.

"7. His belief in the contagiousness of the plague has been proved to be erroneous by the observations of many physicians, particularly by Dr. John Pringle and Dr. Pye, in the beginning of the last century, and by several of the physicians who accompanied the French and British armies in their expeditions to Egypt, in the year 1798.

"8. Many of our author's recipes are founded in an erroneous belief in the efficacy of certain simples, which experience has proved not to possess any virtues, and which, if they did, would be rendered inert by their indiscriminate mixture with each other.

"9. Among the errors of our author, should be mentioned, his belief in the exclusive benefits of experience, and his rejection of theory, or reasoning in medicine. This error is the more to be regretted, as the Doctor reasons nearly in every page of his writings, and thereby establishes an important truth, and that is, that to reason, or theorize in medicine, is not only a natural, but an involuntary exercise of the faculties of the mind.

"Some other errors, of less importance, shall be enumerated in the notes.

"Let not the advocates for the fame of Dr. Sydenham complain of the number of the errors in his writings which have been, or which may be mentioned. They were perhaps wisely permitted to restrain pride in intellectual gifts and attainments, and to console the physicians of succeeding ages for his singular pre-eminence over them, in every department of practical medicine.

"Should it be asked how it was possible for such practical truths to exert a salutary influence in medicine, when combined with the errors that have been noticed? I would answer, by first remarking, that we sometimes observe strong and correct moral propensities and habits to exist in the same mind with the grossest errors in religion. In like manner the uncommon strength and correctness of Dr. Sydenham's talents for observation and reasoning, habitually, and as it were necessarily exerting themselves in his intercourse with diseases, rendered his practice successful, in spite of the counter influence of the erroneous principles of medicine in which he had been educated.

"To enumerate the many truths that are contained in the following work, would be to transcribe, with the exception of a few pages, nearly every part of it. His histories of acute diseases; his details of the laws of epidemics; his intuitive discernment of old diseases, entangled in new ones; his defence of cool air, and of depleting remedies, to which millions owe their lives; his sagacity in discovering the precise time and manner of administering his remedies, and the difference of his practice in the same disease in different seasons, constitute a galaxy of medical knowledge, and mark that rare assemblage of discriminating and combining talents, which have elevated him above the claims of the century and nation in which he lived, and rendered him the physician of all ages and countries. The same talents, employed upon subjects of more general and popular inquiry, would probably have placed him upon the same grade with Sir Isaac Newton, in a scale of human intellect.

"To dispose you still more to appreciate the work which I have the pleasure to inscribe to you, permit me to add, that it is the anchor which has saved our science for more than a century, when it has been driven from its moorings, by erroneous theories, and even by those of our author, as well as of other men. It has moreover been held in the highest esteem by the most distinguished physicians in every part of the world. Dr. Boerhaave has celebrated it in an elegant Latin oration. Dr. Haller has enriched his Bibliotheca Medicinæ with copious extracts from it. Vanswieten's Commentaries upon Dr. Boerhaave's aphorisms owe much of their usefulness to the numerous quotations from it, which he has blended with his account of acute diseases. Dr. Stoll, of Vienna, complained of medicine being a chaos, until he met with Dr. Sydenham's works, which soon changed its apparent darkness and confusion into light and order.

His British annotators, Dr. Wallis and Dr. Swan, seem to have vied with each other in their commendations of them. In the preface of the former to his edition of them, he says, 'they have past the ordeal trial, and stood, unshaken, the test of time, notwithstanding the various doctrines which have crowded into the medical schools, and dropt into oblivion since their first appearance. His writings were the result of observation, and his labours founded on experience; and though he has wandered now and then in the field of speculation, his guide has been simple facts, contending to support certain positions à posteriori by plain reasoning, rather than attempting to prove à priori what would be the result of some imaginary principles; hence, though his arguments may be fallacious, his conclusions cannot be mischievous; because he first enumerates the symptoms of the disease as they appear, then watches the progress, and points out the different modes which have been successful in the different periods. And thus he forms the basis of his practice, exercising his reasoning powers in accounting for the mode in which nature performs the process; or making probable conjectures at the offending cause, how it originates and proceeds, adhering rigidly still to the methods of which experiment has proved the propriety. Indeed, so convinced have later times been of the validity and accuracy of his descriptions, that they are considered as the unrivalled delineations of nature; so universally have they been esteemed for their exactitude and truth, that poets never made freer use of, or stole more from *Homer*, *Pindar*, or *Virgil;* satyrists from *Juvenal*, *Persius*, or *Horace;* orators from *Demosthenes*, *Quintilian*, or *Cicero;* nor dramatists from *Shakespeare*, than physicians have from *Sydenham*.'

"Respectful as this eulogium is upon our author's works, it is not surpassed by Dr. Swan's. After taking notice of the envy and malevolence to which Dr. Sydenham's discoveries had exposed him from some of his cotemporary physicians, he subjoins the following remarks: 'But posterity has made it up to him since his death; for no one, the great Hippocrates himself not excepted, ever stood possessed of a fairer reputation, than he has since enjoyed, and still bears; his judgment, integrity, and candour are universally acknowledged and applauded. The physicians of our own country have recourse to his writings as an oracle, and foreigners never mention him without the most honourable titles: to sum all in one, many of them call him the British Hippocrates.'

"To these testimonies in favour of Dr. Sydenham's works by physicians, I shall add one in favour of his genius, by his cotemporary and friend Mr. Locke. In the preface to his Essay upon the Human Understanding, he classes him with the illustrious Mr. Boyle, as a 'master builder in the commonwealth of learning, whose mighty designs in advancing the sciences, will leave lasting monuments to the admiration of posterity.' And in the conclusion of some elegant Latin verses, which he addressed to him, upon the publication of his Treatise upon Fevers, he tells him, after having extinguished febrile fires, it will require the 'same fire to destroy his fame, which will consume the globe.'

"With all the praise that has been thus lavished upon Dr. Sydenham's works, it is highly probable future generations will esteem them more than the generations that have existed between the time of their publication and the present day; for I have no doubt they contain many apparently useless facts, which will be extensively and successfully applied to the cure of diseases, under the influence of just theories in medicine. I infer this, from several of those which have lately been admitted in our science, having, like the prism which renders objects visible that had pre-existed in an invisible state, already had that effect. I shall briefly mention a few of those theories and facts. A belief in the sedative operation of cold, has rendered popular Dr. Sydenham's application of it as a remedy in inflammatory fevers. A belief in the stimulating powers of opium, has revived his liberal use of that excellent medicine in all diseases of weak morbid action. A belief in the modern doctrine of what has been called indirect debility, or in the depression and weakness of the system from an excess of stimulus, has established his bold and paradoxical practice, of imparting strength to the body, by abstracting blood and other fluids from it; and a belief in life being the effect of impressions only, by directing the attention of physicians to the diurnal revolutions of the system, has shown the propriety of his fixing the precise times for the exhibition of opium, and for the hours of refreshment, sleep, and exercise in certain acute and chronic diseases. It is from an expectation that time will thus continue to bring to light latent and useful truths in Dr. Sydenham's works, that I have preferred publishing the whole to an abridgment of them, lest parts which are now supposed to be uninteresting, should escape the chance of forming the basis or support of future principles in medicine.

" After this account of the great value of the book I have put into your hands, it will hardly be necessary to suggest to you, that it should not barely be read. It should be studied; for there is the same difference between reading and study, that there is between exercise and labour. It should afterwards be read over and over, if you expect to profit by it. It should likewise occupy, in sickly seasons, a place in your parlour window, in order to be consulted more easily in every hour's respite from business. The histories it contains of epidemics, will enable you at such times to contend successfully with similar diseases; for they as far exceed the abstract treatises upon them which are unrelated to time, situation, and other circumstances that are to be met with in books, as correct and faithful travels exceed systems of geography in teaching us the peculiarities of a foreign country.

" The illustrious author of this work, who knew the immense labour with which it was composed, shall furnish a reason for the first part of the above advice. ‘As to the works I have already published,’ says he, in his Treatise upon the Dropsy, ‘if a person will give them a *single* reading, I am sorry I should be the means of making him lose his time; but if he will peruse them *often*, and commit them to memory, I dare say he will reap such advantages from them, as may in some measure equal my wish, and the great pains I have been at in making and compiling them.’

“Should I, gentlemen, by means of this American edition of those works, become the instrument of procuring a compliance with our author's wishes and advice, by the physicians of the United States, I shall not think I have buried the single talent which was committed to me for the benefit of the health and lives of my fellow citizens.

“You will perceive, I have conformed to Dr. Sydenham, and to modern systems of nosology, by using the plural number of ‘disease,’ instead of its singular epithet which I have adopted in my lectures; and that I have not digressed, to explain or defend any of my peculiar principles in medicine, in the occasional references I have made to them in some of the notes. Such a digression would have been foreign to the design of this undertaking, and it is, I hope, rendered unnecessary, by your previous knowledge of those principles, and by the numerous facts in support of them, which you will find in different parts of the work before you.*

“You will perceive further, in the practice of Dr. Sydenham in acute diseases, a justification of that practice which was so generally and so virulently opposed, in the treatment of the malignant bilious fever in Philadelphia, in the year 1793. Its universal adoption by its enemies since that time, is a fresh triumph to our author over prejudice and error, and has served to add the laurels of our country, to those of every other part of the world, which have long flourished around his tomb.

“I have omitted making any remarks upon the processus integri. It was rendered unnecessary from its being an epitome only of the preceding parts of the work.

“I shall conclude this mark of my respect for you, by recommending to your imitation the conduct of Dr. Sydenham as a man and a christian, by which he did as much honour to our profession, as he did by his discoveries as a physician. For an account of many of his amiable virtues, I refer you to the history of his life, which is prefixed to this work. From a great number of passages, I shall select but three, from different parts of his writings, as illustrations of the principles of benevolence and self-denial, and of the modest piety which governed his life. 'Upon deliberate and equitable reflection,' says our author, 'I find it better to assist mankind, than to be commended by them, and highly conducive to tranquility of mind; popular applause being lighter than a feather, or a bubble, and less substantial than a dream.'* 'And indeed, provided I discharge the duty of a good citizen, and serve to the prejudice of my private interest, what matters it, if I gain no reputation thereby? For, upon due consideration, my endeavouring to secure a character, who am now advanced in years, will be in a little while like providing for a non-existence. For what will it avail me after my decease, that the eight letters which compose my name, will be pronounced by those who can no more frame an idea of me in their minds, than I am able to conceive what kind of persons those will be who have no knowledge of their immediate predecessors, and who will perhaps have a different language, and other customs, according to the vicissitudes and fluctuating condition of human affairs?'†

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* We could have wished the Doctor had been rather more explanatory, for as we never had the pleasure of hearing his lectures, this is entirely unintelligible to us, and must be so, we fear, to all who are similarly circumstanced—surely he does not mean there is but *one disease*.

"I will now endeavour," adds the Doctor, in another place, 'to fulfil my promise, by presenting the public with the history and cure of acute diseases; and though, in executing this design, I am sensible I shall expose all the fruit of my labours, for the best part of my life, to the indolent and the ignorant, yet I am too well acquainted with the disposition of this degenerate age, to expect any thing but censure and contumely in return; and know that I should have gained more reputation by advancing some trifling and useless speculation: but be this as it will, I hope I shall be rewarded elsewhere."‡

Although we do not acknowledge the justness of all the opinions expressed in the foregoing extract, we shall forbear commenting upon them, as our observations would not lead to any practical difference. We were very much pleased with the caution which Dr. Rush has prefixed to the processus integri; a compendium in medicine must, in our opinion, necessarily be dangerous except in the hands of those who have no occasion for one.

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* "Treatise upon the Small-pox. † Dedication of the Treatise upon Gout and Dropsy, to Dr. Short. ‡ The Author's preface."

INTELLIGENCE.

DOMESTIC.

A Case of Diabetes Mellitus.

A CASE of Diabetes Mellitus has occurred to one of the Editors of this Journal, in which Mr. Watts's plan of treatment was tried as far as was judged safe, without any benefit. It is somewhat remarkable, that the British Reviewers, in noticing Mr. Watts's publication, are perfectly amazed at the originality of his ideas, and the boldness of his practice. We had supposed, both they and Mr. W. were better acquainted with American literature; for if they had ever read Dr. Rush's Inquiries, they would have found that many of the ideas which they supposed novel, had been published long ago. In reading Mr. W.'s performance, we frequently cast our eyes to the margin, expecting to find a reference to Dr. Rush, as the person who had first promulgated the opinions stated in the text; but, as this was done in no instance, we conclude Mr. W. had never seen Dr. R.'s book. This striking similarity of ideas strongly reminded us of the anecdote of an author who read a part of his play to a friend for his opinion. "Very good," said his friend, "very good indeed, but, pray is not all this in Shakespeare?" "What of that," replied the author; "*it only shows how very much alike* TWO GREAT MEN *may sometimes write.*"

Case of Hernia.

A very uncommon case of Hernia has occurred, of which we shall give the particulars in our next number, where strangulation and subsequent mortification and sloughing of the intestine and integuments took place, attended with regular and natural alvine discharges, until the artificial anus was formed at the abdominal ring.

Transactions of the Duchess County Society.

We have already expressed the high opinion we entertain of the importance of agricultural transactions. To publications of this kind, England undoubtedly is much indebted for her high state of agricultural improvement; it is, therefore, with the greatest pleasure, we see similar works issuing from similar establishments in our own country. In the Preface to the second volume, the committee of publication complain of the small number of original observations with which they have been favoured. This we expect time will remove, and we hope to see a future volume entirely composed of original communications.

With laudable liberality, the society has appropriated the profits arising from the sale of their publications, as premiums for agricultural improvements. Books, however, which are intended to circulate among the people, should be sold low to induce a great number of persons to purchase; which is, indeed, a surer method of securing an ultimate profit, than by charging as extravagant a price as that demanded for Chancellor Livingston's Essay on Sheep.*

Collins & Perkins's Edition of the Anatomy of the Human Body, by John and Charles Bell.

We have the pleasure of congratulating our readers upon the publication of this valuable work, and of assuring them that our anticipation of its execution has been realised. The printing is more correct, and the copperplates, taken collectively, are superior, both in execution and correctness, to those in our copy of the London edition.

The cuts on wood, though not so elegant as those on copper, are equal to them in effect, when both are struck off upon a printed page, so that in this instance, at least, we may fairly boast of the superiority of an *American to a London edition.*

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* When this Essay was reviewed in our last number, the book was not priced. As we have heard so many complaints upon this subject, and have been censured for our unqualified commendation of the work without noticing this circumstance, we insert this note for our justification.

Notwithstanding the popularity and undoubted merit of the work itself, it is by no means free from errors. These are obvious to every anatomist; some have been pointed out by others, and more we could select; but the task would be invidious, and as they are not of great importance, we shall pass them over without comment.

Dr. RODGERS'S REPORT *to the* BOARD *of* HEALTH, *respecting the* FEVER *at* BROOKLYN *last Summer.*

[We were in hopes to have obtained another Report upon the same subject, which is to be made to the Board by Dr. Gillespie, and which, we believe, gives a Statement of the Facts connected with the Introduction of the Fever into Brooklyn, somewhat different from that contained in the following one by Dr. Rodgers. It shall be inserted in our next number.]

New-York, Nov. 29th, 1809.

At a Meeting of the Board of Health, ordered that a Communication of the Health Officer of the 24th instant, together with a Letter from Dr. Smith, of the 20th, to the Health Officer, be published in the News-papers. *J. MORTON, Sec'ry.*

New-York, Nov. 24th, 1809.

GENTLEMEN,

On the occurrence of Malignant Fever at Brooklyn this summer, the question of its origin was agitated, and with it some illiberal reflections thrown on the management of the Health Department.

I then did myself the honour of saying to you, that, at the proper time, I should state such facts as might give light to the former, and should completely refute the latter. I now with much pleasure perform my promise, and I trust that I shall show that all the requisitions of the law have been strictly complied with, and all our duties faithfully fulfilled.

Before I go farther, it may be proper to notice an unfair and disingenuous insinuation against me, that my private theoretical opinions would certainly prevent me from doing my public duty. To every man who is under the high obligations of honour, and more especially the still more solemn obligations of his oath, such a sentiment is vile and detestable.

You know, gentlemen, that the law gives me but little discretionary power; that this discretion is only in the detail of business; that in every material point, it is precise, distinct and peremptory; and whatever it enjoins, I must perform. This, I *know*, has been done; and, with the pride of conscious integrity, I can challenge the contrary to be proved.

I have even done more than I otherwise should, because I knew that it was thought by some, that I did not believe in all the speculative doctrines which the law acknowledges. I had, therefore, prejudice and misrepresentation to avoid, by a steadier and more attentive course, and had to atone for a supposed medical sin, by a work of supererogation.

Although I differ from some on the origin of Malignant Fever in our country, yet I agree with all in the propriety of cleansing foul vessels, of changing bad air, of preventing impure or noxious materials of any kind, or sick persons from going into a city, and of affording a comfortable asylum to sick and unfortunate seamen.

The first section of the law passed in 1804, is what is mostly depended on "to provide against infectious and pestilential diseases;" and to show that this has been properly attended to, I declare, that from the first of June to the first of October, no vessel has gone up to New-York, on board of which has been any Malignant Fever, or person who had had it during the voyage, or which had come from any place where such fever prevailed at the time of her departure; nor has any vessel gone that had had that disease during such voyage; no foul materials of any kind, or dirty clothes, have gone up to the city; nor has any person gone up contrary to the strict letter and spirit of the law passed in 1806, directing the performance of four days of quarantine of observation. Passengers have occasionally given bonds to spend these days at other places than at the quarantine ground; and these obligations have never, that I know of, been violated. No person has deserted from any vessel on board of which has been Malignant Fever. The intercourse with the quarantine ground has been as strict as the nature of the place and the institution could admit; nor has any evil arisen from any improper intercourse.

On this ground, then, I might rest a secure defence. I, however, go farther, and shall consider the cases of several vessels which have been particularly inculpated, because thereby it is wished to criminate the Health Officer. The first is the *Concordia.* This ship arrived at the quarantine ground on the 17th of June, nineteen days from the Havanna, navigated by *twelve* men, with *fifteen* passengers. The Havanna was healthy at the time of her sailing. She brought no disease; the same crew had made the voyage in perfect health, except one, who had slight indisposition for a few days, when he recovered; no one had had Malignant Fever on board; no impure materials were or had been on board; the cargo was molasses and sugar; she staid her four days of observation, and all being well, the ship being ventilated, their clothes being washed, and the bilge-water clean, she was permitted to pass to New-York, with the usual directions to unload and lay in the stream according to law. From this we see that the Health Officer could do no otherwise than permit this vessel to go up; nor had he the right to direct her to any other position than what the law prescribed, nor the power to prevent her from going to Brooklyn, or Jersey, or any other place, other than that portion of New-York island interdicted by law. In doing then as he did, he fulfilled the requisitions of the law; and if he had not so done, he would have violated his duty, have incurred deserved censure, and probably exposed himself to prosecution.

I now give you a statement of facts relative to the Concordia after she left the quarantine ground. It is in part taken from the affidavit of the mate of this ship: "That the ship Concordia, after having discharged her cargo in the stream at New-York, hauled over to Brooklyn on the 29th of June last. The next day a seaman, of the name of Nathaniel Mullen, was taken sick; he had been ashore at New-York, and the day before hauling over to Brooklyn, was excessively drunk the whole of that day, and the following night; while drunk, slept the whole of the night on deck, exposed to the dew and air. On the 30th of June he was sent ashore, and went to the house of Mrs. Smith, in Brooklyn, where he died the 3d of July. Mullen had not been at Brooklyn before the ship hauled over, nor had any of the hands been on Long-Island before the 29th of June, nor had any one from Brooklyn any intercourse with the ship before that time. The people of the vessel visited and attended on Mullen during the whole of his illness. None of them, however, nor any other person who has had any connection with him or the house, either as lodgers or visitors, have had any complaint of a febrile nature. There were at the time of Mullen's death several lodgers in the house, and it has continued to be a lodging house ever since, and no one has been indisposed that has been there. Mullen had been well during the whole of the voyage to the Havanna and home again, and had never been sick since being connected with the ship, till his drunken frolic, and sleeping afterwards on deck. Two men were hired at Brooklyn to clean out the limbers of the ship, and they have been free from any complaint to this time. The limbers could not be said to be dirty; for although the ship had stone for ballast, on which some molasses had drained, yet there were not more than two buckets full taken from her, and that was chiefly sand, and not at all of a bad smell. The ship is 255 tons. No person has ever been on board the ship at Brooklyn, to inquire into any circumstance of her case; nor has any question ever been asked on board of her, respecting her health or that of her crew, but by the Health Officer, at the quarantine ground. The person giving this information has been on board almost constantly; he and three seamen have lived constantly on board, slept and eat there, and none have been in the slightest degree indisposed. Mr. Sands, at Brooklyn, and his wharfinger, were frequently alongside, the latter often on board, and neither have been affected at the time of giving this information.

" Masters and mates of vessels, perhaps one dozen in number, have *very often* been on board, and staid frequently till late at night, so as to make long visits; none of these have ever been in the smallest degree indisposed with fever, or any other sickness that the informer knows. The intercourse with the ship was free and unrestrained. A block-maker, with three of his people, constantly, and sometimes four assistants, worked for many days on the wharf at which the ship lay, and close to her, and were, during that time, *over* and *on* the ballast thrown out of the ship, and none of them have been to this day indisposed. Mullen boarded at Mrs. Smith's, one wharf below where the shipping get water, and below any house where there has been any sickness. Mullen had no clothes washed at Brooklyn (except by Mrs. Smith.) They were all taken by the person who signs and swears to this, put into a chest by him, and those which were clean sent to his friends."

By this it appears, that neither Mullen nor any of the people of the Concordia had ever been at Brooklyn before his sickness, nor had any of the people of Brooklyn any communication with the ship before the 30th of June.

We see also sufficient cause for the production of disease in Mullen; and when we consider it in connection with other cases of fever occurring this season, without any intercourse whatever with shipping, and precisely and exactly similar in their exciting cause, we cannot for a moment doubt respecting his.

By the testimony of Mrs. Smith and others at Brooklyn, we know that Mullen was brought to her house in a boat, and therefore had no communication with any persons belonging to Brooklyn, in passing from the ship to her house, which is at the lowermost part of the town. There were at the time of Mullen's death four lodgers in her house. These men worked at Messrs. Hicks and Jackson's yard, to the westward of Mrs. Smith's, and still farther removed from the infected part of Brooklyn; neither of these, nor any person whatever with whom they have been or lived, have had any sickness.

Mrs. Smith's family and her lodgers had no intercourse whatever with the upper part of the village, nor did any one from that quarter come to her house during the season of sickness, or after Mullen's death. The distance between Mrs. Smith's house and the infected part of Brooklyn, is more than a quarter of a mile. The person of Mullen then appears to be completely free from any blame whatever in producing disease, nor does any one in Brooklyn venture to say that it could possibly be communicated through him. There is now no other possible way for the disease to be produced, on the principles of importation, but by the cargo of the ship or foul air in her. The cargo was, as you have seen, not of materials fitted to convey contagion, or produce foul air; it was sugar and molasses; and even if it had been of other materials, and ever so foul, they were all discharged at New-York, and sent into the city, except a few boxes of sugar, which were retained on board at the moment of hauling over to Brooklyn, because the ship needed them with the ballast to keep her on her legs. These, however, were discharged immediately on coming to the wharf at Brooklyn, and taken in a lighter directly to New-York; and none of the persons taking this cargo on shore, carting it to the different owners, or any other person concerned with it, has ever been sick. How can we now imagine that this cargo could possibly produce evil, even if all the town of Brooklyn had handled it?

From the declarations of those who have survived their illness, and from what the unfortunate victims to the disease, who were asked on that point during their illness, said, it does not appear that a single one of them ever had been on board the Concordia, or had any connection with any of her people. I have seen a list of fifteen, among the first cases which occurred at Brooklyn, and it appears that not one had been on board this ship, though some of them had been on board of other vessels. I have also the testimony of several of the most respectable citizens of Brooklyn, in which they all agree in never having heard any one say, or having heard it said, that any one supposed he derived his sickness from the Concordia. But there would be internal evidence to this effect, for the ship was extremely light, and appeared so to any eye, and therefore no one would ask to unload a ship which was evidently unloaded; nor were there any bales or other goods on the wharf where she lay, as an incitement to the inquisitive labourer, and she lay below and detached from other vessels which had cotton and other goods on board, and which were unloaded by labourers. She was so light and high out of the water, that for the greater part of the tide, going on board was very inconvenient and troublesome; no one therefore but a person having absolute business would go on board. It appears, in favour of the ship, that the first cases of fever were among those who by their occupations had no connection with shipping. But the entire freedom from disease of all those who we know had communication with the Concordia, is evidence in her favour, and against her introducing sickness at Brooklyn. There were *eleven* of the crew, exclusive of Mullen, and *fifteen* passengers; these, when added to the *pilot* and his *two* men, who took him on board, the *Custom-House Officer and his boatmen*, with the *Health Officer and his boatmen*, at the quarantine ground, the *Boarding Officer and his men*, the *Inspector of Customs*, and *lighterers* at New-York, the *one dozen* masters and mates, frequent visitors to the ship, the *labourers* to discharge the cargo, the *limber-cleaners*, the *blockmaker and his assistants*, the *wharfinger and his son* at Brooklyn, will give you an aggregate of between *seventy and eighty* persons, whom we know, and who might be named, who have had free and unrestrained communication with the ship before she was remanded to the quarantine ground. If to these you add the clerks or agents for those who had cargo on board, and who probably did go on board before hauling over to Brooklyn ; the outward crew who came on board at the quarantine; the lighterers employed to bring ballast from New-York ; the pilot and his men to take her out, and others who had business on board, you will have a number far exceeding *one hundred persons* who were on board this ship, *not an individual of whom has been in the least affected thereby*, *but continue*, *as far as we know*, *in perfect health to this day.* If the persons connected with her had been *first* affected, or if *any* of the persons *most* connected with her had *at all* been affected, then there might be some suspicion, and even then it would not amount to any thing like positive proof; but in the present case, there is no ground for even suspicion itself to rest on, and all the proof is in favour of the ship. We have seen that there were no materials to produce foul air from ; and even if there had been foul air in her, it would only have affected those who went below the lower deck ; for the air in the hold of a large ship is generally cooler than the external atmosphere in July, and would therefore seek the kelstone ; and in the hold of a double decked deep ship, would be at perfect rest, and therefore could not infect any one walking over the deck, unless there were a quantity of putrefying materials on board, constantly generating and throwing off this foul air. But all these supposed circumstances, in all their parts and connections, were absolutely and entirely wanting.

An exception has been taken and urged against the Concordia, because the mate had said that he had poured vinegar into the hold upon the ballast, and had washed the forecastle with vinegar, directly after the death of Mullen. It is perfectly natural to suppose that the mate was alarmed when he found Mullen had died of a disease pronounced Malignant Fever ; and, supposing that Mullen had caught it somewhere, he might fear that he had or would communicate it to the ship : with this apprehension he used vinegar, believing it might destroy all contagion. Now, his doing this *then*, and not before, and telling of it, is a proof that he had no idea of any danger or evil being in the ship, except as resulting from Mullen alone; and, instead of being an argument against the ship, is in her favour, and shows that he supposed Mullen had imported evil from the place he had come from to the ship, and that place was New-York. We know that sailors always wash out the forecastle or steerage of a ship whenever any one dies from her, and when the vessel is hove out they go through a process of cleaning the ship. This is done as well from the fear of contagion, as because the neglect of it might operate against the vessel and lazarettoes or quarantine establishments. The process gone through of itself changed the state of the air, and thereby would render intercourse with her more secure.

It surely will not be said that the ballast gave any disease. This was clean stone ballast, on which some drainings of the molasses had come; it was thrown out at Brooklyn; it was by no means offensive; nor could it be so, from the circumstances in which the ship was, and the washings just now related. It was so far from being disagreeable or inconvenient to be on, that it appears five people who were constantly working on the wharf where it lay, were frequently *over*, and often working directly on it. It deserves to be remarked, that the buildings and lots which were nearest the Concordia, and which form the north-western face to the wharf where she lay, are Mr. Sands's store, the end of Mr. Nichols's bakery, the blockmaker's shop, and the end of Mr. Nichols's office. Now, *all* the persons, and the *only* persons, to the amount of *nineteen*, who worked nearest, and were on this spot, have continued well. This most certainly does not look as if there was foul air or disease in the ship or ballast. Thus, while we are looking for accusations against this vessel, we find that the nearer we approach her, the more health we see, and the farther we recede from her, the more disease we meet. The perfect impunity then with which all the persons that we know of had connection with this ship, the entire safety with which they dwelt in her, and their continued freedom from all disease, puts the question of the air of the Concordia and the whole matter completely at rest.

We now come to the *Brutus*. This ship was also implicated, though I cannot see for what reason, unless it was to throw a little more blame on the Health Officer, in having permitted a vessel to pass to the city, supposed to be sickly; and if there had happened to be any Malignant Fever in the neighbourhood of where she lay, it would no doubt have been attributed to her. She would then be charged with importing it from Brooklyn, where she had been three days, and thus, with additional blame, some public resentment might be turned against the Health Officer. These, by the favour of heaven, have happily not occurred. The Brutus plainly came from a wrong port, and arrived too late to contribute any thing to the mischief at Brooklyn; and the neighbourhood of the Old-slip has continued remarkably healthy. This ship arrived on the 12th of July from Savannah; she was healthy, and had been so the whole voyage; Savannah was healthy, and has continued so to this time; her cargo was rice and cotton; she went up immediately, and hauled into Brooklyn that evening or the next morning; she discharged her cargo on the 13th, 14th, and 15th, and on the 16th hauled over to New-York. She had no connection with any vessel at Brooklyn then or any time since, nor the ship any connection with any vessel from the West-Indies then or since. From the 16th of July till the 26th of that month, when she was ordered to the quarantine ground, she lay between the Old-slip and the Coffee-House-slip. On the 20th of July, John Pitman, a seaman, was taken sick with fever; he was but slightly affected, and was convalescent when the ship came from New-York. On the 21st, Henry Jenkins was seized with fever, and came down to the Marine Hospital with the ship; he died on the 31st of July. Jenkins had the same fever which has affected so many seamen from Savannah river this season, and which has occurred in vessels from that port every year since I have been at the quarantine ground. It will not, I presume, be said that Robert Eastes, the seaman who fell into the hold of the Brutus, and died in less than forty-eight hours afterwards, had Malignant Fever; and yet even that was asserted by some not knowing all the circumstances of the case. The Brutus had no other febrile sickness or complaint on board, and clearly did no harm. Two of the persons who had fever at Brooklyn, said that they had worked in storing cotton from the ship *Alligator*, and had worked in her hold. This circumstance was only stated by them incidentally, without attributing any particular blame to her. It however is urged by some persons in Brooklyn as a cause of complaint. This ship could not import fever, because she came from a port where there was no sickness, nor had any one been sick on board, nor were there any foul materials to produce disease. We absolve then the Alligator from all blame. Two men also are said to have been taken sick after working on board the ship *Joseph and Phœbe*, and one of them said that the ballast was foul. This vessel sailed from New-Orleans in May, and arrived at quarantine on the 20th of June last, with a cargo of cotton and tobacco. New-Orleans was healthy at her departure, and she brought a bill of health. Her crew consisted of sixteen, with eight passengers, all in health: these have all continued well: this vessel went up on the 24th of June. I am not prepared to say what was the state of her ballast at the time the above-mentioned men worked on board, because this ship has since gone to Europe, and I know not where to obtain further information respecting her. I have not, however, heard of any one suspecting harm of her, except the man first alluded to. I do not believe the ballast was foul, or it would have indicated itself by the state of the bilge-water, or some smell in the forecastle and steerage; neither was the case. But allowing that the ballast was foul, and allowing that there had been a sufficient quantity of putrid materials on board to have produced sickness, yet this would not add an atom of weight to the argument of importation in her case; it would only go to prove that local evil in a ship would produce disease just as in a house. The very principles of importation release this vessel from blame, and the entire freedom from disease of every person connected with her, (except the man from the poisoned air of Brooklyn,) goes to show that no evil resulted from her ballast. The bare circumstance of an individual being sick after having been in a particular spot, is no conclusive argument against *that* particular spot; there must be an union of a great many circumstances to establish such a point. If *every one* working there were taken ill, and *they only;* and if *that* spot or place *only* was foul, and all other places clear in which these persons lived or worked, then you might attach suspicion to such spot; but directly the reverse is the fact in the present instance; for, added to the testimony in favour of the vessels implicated, you have the most unquestionable proof of the number and magnitude of local nuisances at Brooklyn, sufficient to poison the purest atmosphere, and engender the most fatal pestilence, and also, of *the very persons* who had worked on board these vessels living in the filthiest spots, lodging in the most sordid and crowded apartments, and indulging in habits every way fitted to destroy them. You will hardly credit the assertion, yet it is nevertheless true, that in the middle of July last two of these very men were visited in a small and extremely confined garret, not fourteen feet from the surface of the ground, belonging to a house, the cellar of which had been filled with all imaginable filth for some years past, and was at the time some inches deep with water and mud, emitting a noxious and pestilential air; that this garret had no window to it, and had light and air only admitted by a small scuttle, which was shut whenever it rained; and that the person who gave me the information was under the necessity of having a candle to light him through the room at noon day. In this wretched apartment, rendered deadly by the foul air from the cellar and adjoining yard, were lodged three miserable men, whose own confined breath made pestilence more fatal. I need not say that two of them died, and the third survived by miracle! These men would have had Malignant Fever if there had not been a ship within a degree of New-York.

The last vessel I shall notice is the ship packet Pizarro. I should not consider the case of this ship, if it had not been said that she was a foul ship when she came up, and thereby some blame thrown on the quarantine examination, and on the master of the vessel. The truth is, that the packet Pizarro was, when she went up to New-York, as clean a ship as any in the harbour, and yet some good and worthy men, having heard a suggestion of danger respecting her, gave information against her as a foul ship and a grain vessel; and although neither of these was the fact, the Board of Health were obliged, to gratify public opinion, to order her to the quarantine ground, where, after discharging her cargo, heaving out her ballast, and cleaning out her limbers, it was fully proved that all the charges were utterly and entirely groundless.

It now appears, from the view which we have taken of this subject, that those persons most connected with shipping, and those living on board of ships, have been entirely free from disease, while those removed from them have been affected. This is utterly inexplicable upon the supposition of importation, connected with the known principle of contagion being more concentrated and more active the nearer we approach the body therewith affected, or the source and centre of action. Upon such supposition, the farther from danger the greater would be the dangerous effect.

We have also seen this season, that the disease in question has never been personally communicated, or transmitted beyond or from the individual affected by the poisoned atmosphere; and that, although the intercourse between Brooklyn and New-York was open and unrestrained for more than a month after the disease appeared in the former place, no importation took place to the latter; and even when the communication was interdicted by ordinance, we know that the law was openly evaded, and the intercourse constantly kept up, yet no evil has happened. It surpasses then, all human conception, to suppose that a vessel, in the circumstances of the Concordia, or others just named, could possibly produce disease, and yet the very subjects of the disease itself not be able, in a single instance, to affect others; or, that this disease should come, in the persons of healthy men, in a healthy ship, in pure and clean materials, more than a thousand miles, and yet not be able to pass over as many yards, in the bodies and clothes of hundreds of people of all descriptions, pure and impure, sick and well, passing and repassing constantly every hour in the day, for more than a month, to and from the very centre of contagion—such a supposition would involve the gross absurdity of the infinitely lesser cause producing the infinitely greater effect, or the infinitely greater cause producing no effect at all. Having now, by a strong chain of circumstantial evidence, proved that the disease was *not* imported, and there being a total want of any in the affirmative, we are compelled of necessity to allow it to be of domestic origin. It is indeed now admitted, on all hands, that Malignant Fever may, and often does, arise from local and domestic causes. This is allowed by the Board of Health of Philadelphia this summer, in the case of two men sickening from the influence of damaged fish; and we suppose it to be of domestic origin in the case of the man arriving from Philadelphia, and who died on Governor's Island in August last; we know also that two men with Malignant Fever, were sent last summer from New-York to the Marine Hospital, neither of whom had any connection whatever with shipping or seamen; one of them was seized on the 5th of July, after being excessively drunk and exposed one whole night to the open air, *precisely and exactly similar to the case of Nathaniel Mullen, of the Concordia.* After the summer and autumn of 1805, from the utter inability of proving the importation of fever *then*, and the abundant number of facts going to establish the opposite truth, no one doubted the disease to be of domestic origin *that* year; and so strong was this belief, that an attempt was seriously made to reconcile the two doctrines, by admitting of domestic origin *occasionally:* all this being admitted, and it being also allowed by the Board of Health of Charleston (S. C.) that Malignant Fever this summer in that city was not imported, but *endemial,* we may surely suppose it *might* be produced in Brooklyn this season too. This supposition is, however, strengthened into firm belief, when we consider the report of the Brooklyn Committee, and the several relations which you have had of the nature and extent of the nuisances of the infected part of that village. We know, in conformation of this opinion, that in no instance has this disease ever appeared as epidemic in any of our cities or towns, without the presence of open evident local nuisances. In addition to what has been said, I beg leave to refer you to the enclosed letter of Dr. Smith on that subject.

I have now, gentlemen, effected my purpose, and have not stated all the facts which would still more confirm it. The limits of this letter forbid my going into the contested doctrine of fever and its production. You will see this subject well discussed elsewhere; and if I were to point to a production where it is treated in a manner the most closely condensed, yet most luminously clear, it would be to the letter of the Resident Physician, addressed to Governor Lewis, in January, 1806. This is among the documents of the Health-Office. It only remains that, with you, I offer up the humble tribute of gratitude and praise to the Father of all mercies, for the uncommon health of our city during the last season, and for that unvaried freedom from pestilence with which we have been blessed for four successive years. With much respect,

I have the honour to be,
Your obedient servant,
JOHN R. B. RODGERS,
Health Officer.

The Hon. De Witt Clinton, Esq. President, and members of the Board of Health.

Letter from Dr. Ephm. Fitz R. Smith, *relative to the Fever at Brooklyn, dated November*, 1809.

Dear Sir,

According to promise I now furnish you with those few facts, relative to the source of contagion in the late Malignant Fever at Brooklyn, that have come under my view.

You will recollect that the month of June, with the exception of some days in the latter part of it, was uncommonly cool. A good deal of rain fell early in the month, which on the 9th and 12th was very heavy. About the 19th it began to grow warm, and from this time till the 29th the mercury stood higher than it has since done through the whole summer. About this time the fever broke out, certainly under circumstances very favourable to domestic origin. For although from the situation of Brooklyn, and from the smallness of the place, we should not expect to meet with many sources of miasmata, yet the fact was, that nuisances of every description abounded, and at a time too very favourable to noxious exhalations. I shall not rest satisfied with mere general assertions of the filthiness of the streets, though these were very dirty, but shall proceed to point out those particulars which came under my inspection.

About midway between the old and new ferries there is a small street or lane, which comes down the hill obliquely, running about N. W. and S. E. This lane is very crooked, and in some parts comparatively low, so that there are many obstructions to the draining off of water, although on the side of a hill. It is occupied chiefly by small houses and stables, and not being a public place, there was no destruction nor removal of perishable substances, however great the quantity. Accordingly, at different distances up this lane there were small heaps of animal and vegetable matter thrown from the adjoining houses, and many of them in a state of putrescency. The stables were very dirty and very offensive to the smell. So that in walking up this street the senses were continually annoyed. But however abundant filth might be here, it was comparatively small to what might be found lower down. At the foot of the lane is a wharf (I believe Sands's wharf, next below his house) on which are situated a number of small houses, and among the rest four small wooden houses, chiefly occupied as sailor's boarding houses.

Here was to be found the great source of disease: for being situated on a flat at the bottom of a hill, all those substances, which escaped from above, were washed down, and here accumulated in a stagnant heap. Not only the wharf and street were very dirty, but the cellars of these houses were receptacles of filth subsiding from above. One of the houses, occupied by M'Gee, I inspected narrowly, and such a mass of putrescent matter as was collected in the cellar I never before saw. It had long been uninhabited, and from its low situation it was constantly filled with water. Here were to be found animal and vegetable substances of every description collected in a great mass, and constantly covered with water. The fermentative process was so perceptible to the eye, that the surface was covered with bubbles continually bursting and emitting a gass intolerable to the smell. The slops of the house were thrown into the cellar, and every other nuisance of a sailor's boarding-house was here deposited. You can readily conceive that thousands of poisonous vapours might exhale from this cellar, and that not only a small village but a large city might be infected by such a cause. But this is not the only source of contagion. The cellars of the adjoining houses, I was informed, were nearly as bad as this; and from their external appearance I could readily believe it, for they were on the same level, and the houses above occupied by the same class of people. But, moreover, in the rear of these houses, at the distance of a few yards, is a brewery. This brewery is in a very low situation, and so conscious was the brewer of the filthiness of his place, that upon asking some questions relative to his recovery (for he had had the fever,) and what he supposed to be the cause of it, he directly assured us there was no dirt about the brewery, and that we need not seek for any other cause than importation; but, upon inspection, we found that he kept a number of swine, which were fed from the washings of his brew-house, and that an immense mass of filth lay directly below their pen. This was very offensive, and annoyed the whole neighbourhood.

Thus then, if we combine these various sources of contagion, and consider that they were all found nearly in the same place; and if to these we add the filth taken from an adjoining dock, and deposited upon the wharf nearly about the same time; and further, if we advert to the facts that the disease originated in, and was almost entirely confined to this neighbourhood; I think we need not seek further for the cause, but may rest satisfied that in this instance at least our own country is to blame for the production of this pestilential fever. I have purposely avoided saying any thing relative to the suspected vessels, because the facts already in your possession are more minute, as well as more conclusive, than any that I could furnish you with. If these will be of any service to you, you are at liberty to use them as you please. I am sir, yours, &c.

EPHM. FITZ R. SMITH.

Dr. RODGERS.

To the Editor of the Mercantile Advertiser.

SIR,

In your paper of the 8th instant is published a letter from me to Dr. Rodgers, on the subject of the Brooklyn fever. I beg leave to correct an error therein stated. I have just been informed that the house containing the principal nuisance was not occupied by M'Gee, but by some other person. M'Gee's house is directly opposite.

Dec. 12. EPHM. FITZ R. SMITH.

Officers of the Medical Society of the city and county of New-York.

At an Anniversary Meeting of the Medical Society of the county of New-York, held on Monday, the third July, the following gentlemen were elected Charter Officers for the ensuing year:—

Dr. James Tillary, President.
Dr. William Moore, Vice-President.
Dr. John Onderdonk, Treasurer.

Dr. W. W. Buchanan, Secretary.

Dr. J. R. B. Rodgers, Dr. William Hammersley, Dr. John C. Osborn, Dr. Archibald Bruce, Dr. John Neilson,	Censors.

Regular Meetings of the Officers of said Society will be held on the last Wednesday of every month.

Persons desirous of being lincensed for the Practice of Physic and Surgery, must apply to either of the Censors at least ten days previous to such meeting.

W. W. BUCHANAN, M. D.
Secretary of the Med. Soc. New-York.

Circular from the Faculty of Physic of Columbia College, to the Presidents of the Medical County Societies.

New-York, July 15, 1809.

SIR,

The Faculty of Physic of Columbia College, wishing to disseminate Medical Instruction, and desirous of co-operating with their professional brethren incorporated under the law regulating the Practice of Physic and Surgery in this State, have adopted the following resolutions, which I am requested officially to communicate to you.

1st. That they will admit to the course of Medical Lectures given in this College, one Student recommended by each of the Medical County Societies in this State; who shall present his Certificate of such recommendation from the President of the County Society, or from any other person it may designate, to the Secretary of this Faculty.

2d. That any Student of Medicine who may wish to obtain the Medical honours of this Seminary, on producing proper testimony that he has studied Medicine for the term of three years with a legal Practitioner, as well as all those who may have obtained a license to practise Physic or Surgery from any of the

Medical County Societies, shall be entitled to one year's standing in the College. The term by this means becomes abridged from three to two winters, during which it will be expected that they attend at least one course of all the Lectures on the different branches of Medicine.

In making this communication, the Faculty are happy to add, that they are not only supported by, but have received the approbation of the regular Academic Authority of the State, and that on no occasion has any Student been refused the benefits of this Institution, when, on a candid representation, it appeared that he was unable to defray the usual charges; but that, on the contrary, every exertion has uniformly been made to facilitate such means of information as might render him successful in his profession, and useful to the public.

By Order of the Faculty.

JAMES S. STRINGHAM, Secretary.

Bruce's American Mineralogical Journal.

Archibald Bruce, M. D. Professor of Mineralogy in the University of the State of New-York, has published the prospectus of a periodical work, to be entitled the American Mineralogical Journal.

The object of this work is to collect and record such information as may serve to elucidate the Mineralogy of the United States, than which there is no part of the habitable globe which presents to the mineralogist a richer or more extensive field for investigation.

Of the utility of a publication of this kind, much might be said: it may however be sufficient to observe, that nothing has contributed more to increase and diffuse Mineralogical information than the periodical works on the continent of Europe, particularly those in Germany and France. At the present period, when such laudable exertions are making to improve and extend the manufactures of our own country, a knowledge of the mineral productions, on which so many of the useful arts depend, and with which nature has so liberally supplied us, becomes particularly desirable.

The Mineral combinations which exist here, under forms unknown in Europe, will, we trust, procure for this Journal some attention abroad.

In order therefore that the design may be carried into effect, communications from those gentlemen who may have directed their attention to this interesting branch of science, are respectfully solicited, particularly such as may relate to the Geology and Mineralogy of particular districts; the history of Mines, their products, methods of reduction, and improvements in Metallurgy generally; descriptions of individual specimens, their constituent principles, localities and uses to which they may be applied in the arts; mineral waters, their situation, analysis and use in the arts, and in the cure of diseases, &c. A part of the work will be appropriated to such useful information as may be derived from foreign and domestic journals.

FOREIGN.

Analysis of James's Powder.

Mr. C. L. Cadet found that 19 parts of this powder, which he analysed, were composed of

	Parts
Oxide of antimony at a maximum of oxidation	7
Phosphate of lime	4
Sulphate of potash	4.5
Free potash holding oxide of antimony at a minimum	3.5
	19

To recompose the powder we must take	Parts.
Sulphuret of antimony	4
Calcined phosphate of lime	3
Nitrate of potash	8

These being powdered, mixed, and triturated together, are to be put into a crucible, which is to be covered and exposed to a strong heat. The white powder in the crucible is the same as James's Powder.

Mr. Pully, informed Mr. Nicholson, from whose journal for April this is extracted, that he had analysed this powder to compare it with that of Dr. James; and that he has found it to contain the same principles, and in the same proportion.

Messrs. Allen and Pepy's Experiments on Respiration.

These gentlemen have performed a suite of experiments upon respiration, from which they have drawn the following conclusions. The experiments are detailed at too great a length to be inserted in our journal, but from the construction of the apparatus, and the great attention of the experimenters to avoid every source of error, there can be little doubt of their accuracy.

1. It appears that the quantity of carbonic acid gas emitted is exactly equal, bulk for bulk, to the oxygen consumed, and therefore there is no reason to conjecture that any water is formed by an union of oxygen and hydrogen in the lungs.

2. Atmospheric air once entering the lungs returns, charged with from 8 to 8. 5 per cent. of carbonic acid gas; and when the contacts are repeated almost as frequently as possible, only 10 per cent. are emitted.

The 12th and 13th experiments prove that when the inspirations and expirations are more rapid than usual, a larger quantity of carbonic acid is emitted in a given time, but the proportion is nearly the same, or about 8 per cent. The proportions of carbonic acid gas in the first and last portions of a deep inspiration differ as widely as from 3. 5, to 9. 5 per cent.

3. It appears that a middle sized man, aged about 38 years, and whose pulse is 70 on an average, gives off 302 cubic inches of carbonic acid gas from his lungs in 11 minutes; and supposing the production uniform for 24 hours, the total quantity in that period would be 39534 cubic inches, weighing 18683 grains, the

carbon in which is 5363 grains, or rather more than 11 oz. troy. The oxygen consumed in the same time will be equal in volume to the carbonic acid gas, but it is evident that the quantity of the carbonic acid gas emitted in a given time must depend very much upon the circumstances under which respiration is performed; and here it may be proper to notice, that all the experiments were made between breakfast and dinner.

4. When respiration is attended with distressing circumstances, there is reason to conclude that a portion of oxygen is absorbed; and we remark that as the oxygen decreases in quantity, perception gradually ceases; and we may suppose that life would be extinguished on the total abstraction of oxygen.

5. A larger proportion of carbonic acid gas is formed by the human subject from oxygen, than from atmospheric air.

6. An easy natural inspiration is from 16 to 17 cubic inches in the subject of these experiments, who makes about 19 in a minute. This, however, will vary in different individuals; and perhaps we ought to estimate the quantity of carbonic acid gas given off in perfectly natural respiration at somewhat less, and most likely at considerably less than in the statement above, when we consider, that in short inspirations the quantity of air which has reached no farther than the fauces, trachea, &c. bears a much larger proportion to the whole mass respired than when the inspirations are deep.

7. No hydrogen, or any other gas, appears to be evolved during the process of respiration.

8 The general average of the deficiency in the total amount of common air inspired appears to be very small, amounting to only .006; and we are inclined to attribute it in great measure to the difficulty in exhausting the lungs as completely after an experiment as before it, the first expiration being made into the open air, the last into the apparatus.

9. The experiments upon oxygen gas prove, that the quantity of air remaining in the lungs and their appendages, is very considerable, and that without a reference to this circumstance

all experiments upon small quantities of gas are liable to inaccuracy. *Nicholson's Journal for March.*

Berthollet's Experiments.

From some experiments detailed by Mr. Berthollet, junr. it has been conjectured that hydrogen is the base of charcoal. This hypothesis satisfactorily explains the phenomenon of plants growing in water. *Ibid. for May.*

Account of a new Theory of Electricity, from an Essay on Electrical Attractions and Repulsions. Jourl. de Physique, *T.* 63, p. 378. and Phil. Jour. *V.* 22. p. 308.

The late discoveries of the more extensive agency of electricity in natural phenomena, than was formerly supposed by its most sanguine admirers, renders any information of importance which tends to throw any light on its nature. The theory which the anonymous author of this essay advances has the merit of great simplicity to recommend it, of which he justly observes, that in general it is found the more we simplify our theories to account for natural phenomena, the nearer we approach the truth.

This new theory consists in rejecting repulsion entirely from the electrical system, and solving all the phenomena hitherto ranged under that name, by the operation of the superior attraction of surrounding bodies, or vapours. Metal has a strong attraction for electricity, therefore a bit of foil is drawn to a charged glass tube, but instead of being repelled from the tube again, as is supposed, it is only drawn away by some other body, that has a strong attraction to the charge of electricity it has just received, which charge, at the same time, puts an end to its attraction for the electrical tube; and if its gravitation is prevented from acting, by suspending it, or any other method, it will, when it has delivered its charge to the other body, return to the tube for another charge, and pass alternately from one to the other till it has brought them both to the same state of electricity. The apparent repulsion of the pith balls in the electrometer, the author accounts for by their being attracted by the aqueous vapours dissolved in the air, while they have no attraction for each other, from being equally charged. The divergency of the fibres of a tuft of hair or thread, when placed in a charged conductor, he also accounts for, by the agency of the same aqueous vapour.

To understand the operation of the aqueous vapour, it must be considered that any part of it is capable of receiving a charge of a certain strength, without communicating it immediately to the rest, and thus the balls are surrounded with an electrical atmosphere where the attraction of the vapour ceases; while beyond this space it acts powerfully on the charged balls, which tend therefore to pass to the surface of this charged space; but as they separate, the atmosphere of each tends to assume the spherical form it would have, if entirely detached from the other; therefore, when the separation has proceeded to a certain degree, the attraction of the surrounding vapour will operate between the spheres of electrical atmosphere of the balls, in the part beyond the point of contact, and will then begin to draw them towards each other again, so that they cannot separate beyond a certain degree, which must depend on the strength of the charge they have received.

The experiment of small bodies made to leap between an electrified plate, and one not electrified, is still more easily accounted for in this method; as is that of the pith balls leaping against the sides of an electrified glass goblet held over them; and also the experiment of the electrical chimes; and several others of the same sort; for in all these experiments the unelectrified fixed body, which attracts the moveable bodies after they have been electrified, and deprives them of their charge, is at once apparent, and in most of them forms a part of the apparatus.

The author in like manner accounts for the experiment in which thin hollow glass globes are made to revolve by being placed in contact with the external edge of a brass ring (about a foot in diameter) suspended horizontally from the charged conductor of an electrical machine, at such a distance from a metal plate as will prevent the glass globes from passing under the ring. Glass becomes a conductor when charged to a certain degree; and the motion of the globes is at least as easily accounted for by supposing them to be attracted by the plate, in conveying the electrical aura to it from the ring, as by supposing them to be repelled by the ring.

The author advances this theory without seeming to be thoroughly convinced of its truth; he observes, that he may be deceived in his reasoning on the subject, and that his manner of seeing the phenomena, may perhaps be by the *idola specus* of Bacon. His object is truth alone, and he concludes with the well-known quotation addressed to the reader,

————Si quid novisti rectius istis
Candidus imperti.———

Of this last-mentioned experiment it should be noted, that how either attraction to, or repulsion from the ring or plate, should give motion to the balls, is not by any means apparent; to effect this, their action must take place more at one side of the ball than the other, but that it does so has not yet been proved. The subject is well worth further investigation.

Of the apparent Radiation and Reflection of cold by means of two concave Metallic Mirrors, by Mr. John Martin.—Phil. Jour. *V.* xx. *p.* 341.

The apparent radiation and reflection of cold by means of two concave metallic mirrors, has usually been accounted for by supposing that the thermometer placed in the focus of one of the mirrors, acts as a heated body, and that the heat radiating from it is transmitted to the cold body in the opposite focus. Mr. Martin objects to this theory, that the thermometer is not in fact a heated body, since it is not hotter than the surrounding atmosphere, and consequently cannot radiate heat; but it has been said, that the surrounding air becomes cooled, and that consequently the thermometer in respect to it, is a hot body, and radiates heat accordingly; this however does not explain clearly why the thermometer should be reduced to a temperature lower than the air which surrounds it.

The new method of accounting for this fact is as follows. The heat enters the cold body, placed in the focus of one of the mirrors, from the surrounding air in all directions, consequently every point of the surface of the mirror becomes cooled, and those points can only be supplied with fresh heat in parallel rays in a direct course from the opposite mirror, because rays only (striking against so many imaginary tangents of that mirror) can be thrown off towards the cold body; the opposite mirror therefore becomes cool, and for the same reason the whole surface of it must be supplied with heat from the thermometer, which consequently must become cooler than a body placed any where in its neighbourhood.

Chinese plan of propagating Fruit Trees by abscission, by Dr. James Harrison.

The Chinese, in place of raising fruit trees from seeds or from grafts, as is the custom in Europe, have adopted the following method of increasing them.

They select a tree of that species which they wish to propagate, and fix upon such a branch as will least hurt or disfigure the tree by its removal; round this branch, and as near as they can conveniently to its junction with the trunk, they wind a rope, made with straw, and smeared with cow dung, until a ball is formed five or six times the diameter of the branch: this is intended as a bed into which the young roots may shoot. Having performed this part of the operation, they immediately under the ball divide the bark down to the wood for nearly two thirds of the circumference of the branch, a cocoa-nut shell or small pot is then hung over the ball, with a hole in its bottom, so small that water put therein will fall only in drops; by this the rope is constantly kept moist, a circumstance necessary to the easy admission of the young roots and to the supply of nourishment to the branch from this new channel. During three succeeding weeks nothing farther is required, except supplying the vessel with water. At the expiration of that period one third of the remaining bark is cut, and the former incision is carried considerably deeper into the wood, as by this time it is supposed that some roots have struck into the rope, and are giving their assistance in support of the branch. After a similar period the same operation is repeated, and, in about two months from the commencement of the process, the roots may generally be seen intersecting each other on the surface of the ball, which is a sign that they are sufficiently advanced to admit of the separation of the branch from the tree; this is best done by sawing it off at the incision, care being taken that the rope, which by this time is nearly rotten, is not shaken off by the motion; the branch is then planted as a young tree.

Dr. Harrison tried this plan at Prince of Wales's Island, and found it to answer. One great advantage to be derived from it, particularly in trees of slow growth, is that the fruit is produced so much earlier than in the ordinary method. The spring of the year, just before the leaves put out, is the best time for performing the operation.

While on this subject we may state that a gentleman in England has discovered a method of making grapes ripen earlier than they commonly do, by cutting out from an eighth to a quarter of an inch of the bark all around the vine, in the month of July, or as soon as the berries are completely formed, and about the size of shot. This operation he says does not injure the vine, but makes it produce grapes not only earlier but much finer. Care must be taken that none of the bark be left in the space above mentioned, as the smallest portion would defeat the intention of the experiment.

Meteoric Stones.

On the 13th of March, 1807, a large meteoric stone fell in Russia; it was accompanied with a loud clap of thunder. On the 19th of April, 1808, a large quantity fell in Italy, in the commune of Pierre de Casignano, in the department of Taro (formerly the duchies of Parma and Placentia). The air was calm, the sky serene, but with few clouds.

Nicholson's Journal for July.

Davy's Experiments.

In the Bakerian lecture, read in December last before the Royal Society, Mr. Davy has given an account of various experiments on ammonia, sulphur, phosphorus, charcoal, the diamond, plumbago, the fluoric, boracic, and muriatic acids. The results seem to prove,

1. That ammonia contains oxygen, which he had ventured to infer from facts related in his Bakerian lecture for 1807, and that in the action of ammonia and potassium, it is the ammonia that is decomposed, and that there is not the slightest evidence in favour of the existence of hydrogen in the metal of potash.

2. That sulphur and phosphorus contain hydrogen and oxygen, and that they are probably combinations of these matters with bases which have never yet been obtained pure, and that they are analogous in their constitution to the oily bodies, except that these last have for their bases the carbonaceous element.

3. That charcoal, in its purest form, contains hydrogen, and the diamond probably a small quantity of oxygen, and that the purest form known of the carbonaceous element, seems to be in plumbago, where it is alloyed with iron.

4. That the fluoric acid may be decomposed and recomposed, and that its basis is analogous to the sulphureous basis.

5. That the boracic acid is likewise susceptible of decomposition and recomposition, and that it furnishes a peculiar basis, which is more analogous to charcoal than to any other species of matter.

6. That muriatic acid gas, in its common form, contains at least one third of its weight of water, and that when the substance is obtained free from water, it is more conducting as to electricity, does not redden vegetable blues, and in different

combinations detonates violently with potassium. Whether the muriatic basis be separated in these explosions, Mr. Davy has not yet been able to ascertain.

In a communication read before the Royal Society, February 3d, Mr. Davy has detailed various experiments on the distillation of substances, formed by the action of potassium on ammonia, which he conceives cannot be explained on any other supposition except that nitrogen is a compound of oxygen and hydrogen, or that ammonia and water consist of the same kind of ponderable elementary matter; whichever conclusion may be finally adopted, the decomposition of nitrogen in the process is sufficiently evident. *Nicholson's Journal.*

Memoir on Fiorin Grass.

[*If the Fiorin Grass really possesses the properties which the following Memoir states it to do, it would undoubtedly be the most valuable substance which could be introduced into our country. Its capability of withstanding " extremes of wet and drought," would render it invaluable to the Southern States, where these opposite conditions of the earth so frequently prevail.*]

In laying before our readers an account of this remarkable grass, (and if it possessed but half the valuable properties described by Dr. Richardson, it would still deserve the most serious attention, not only of individuals, but even of the legislature), we shall make an indiscriminate use of the present, and of a former memoir on the same subject, contained in the sixth volume of the Communications of the Board of Agriculture, and written by the same author. The former memoir was communicated to the Agricultural Society at the request of Mr. Davy, who witnessed the remarkable characters of this grass on its native spot; and, we are persuaded that this circumstance will excite additional interest respecting its history.

The term Fiorin, by which the native Irish distinguish this grass, Dr. Richardson is, somewhat fancifully perhaps, inclined to derive from the words *fave* [grass] and *reem* [butter] observing, with respect to this etymology, that to his knowledge the term "butter grass" is most deservedly applied to the Fiorin. But lest our readers should be carried away by the idea that this grass possesses the properties of the Phulwarah, or "butter tree" of India, it is right to inform them, that the butyraceous quality of the Fiorin does not show itself till the juice of the grass has passed through the lacteals and mamillary glands of the cow; and then not without the aid of a churn. The butter, however, that is thus ultimately produced from it is remarkably excellent. The Fiorin is supposed to be the Agrostis stolonifera of Linnæus. But, as this point does not seem to have been accurately ascertained, and as Curtis, in his Practical Observations, says, that he has experienced more difficulty in ascertaining the several species of the Agrostis, than of all the others put together; we subjoin the following description of it.

Each plant consists of numerous strings [*stolones*] which are immediately connected with the root; and these strings are knotted or jointed at intervals, of from three to five inches. From each joint a thin, grassy envelope issues in the direction of the string; within which, lateral sprouts shoot forth, nearly at right angles, to the joint. These sprouts, together with the extreme point of the strings, are of a most lively green colour. The strings themselves are much paler at all times, and in March are nearly white. The envelope withers as soon as it has discharged its obvious office, of protecting the advancing sprout from the effects of the weather, and gives the whole a more decayed appearance than might be expected from its quantity, being itself a very thin membrane. The strings, which are the essential part, and constitute nine tenths of the crop, vary in length from three to seven feet; but are usually between four and five feet long. Their number is sometimes very great; and in one instance Dr. Richardson found one hundred and forty issuing from one spontaneous root, each of which had six buds. If the joints touch the ground, or even the damp mat formed by the intertexture of the strings, a sprout shoots upwards, and fibres strike downwards and form a root. Each joint is, therefore, a set, from which the plant may be propagated. So that the spontaneous root above mentioned, produced eight hundred and forty sets.*

The foregoing description corresponds in many points with the Durva, or, as it is commonly called, the Dub of India. And Dr. Richardson says, that his friend, Colonel Macan, who commanded the British cavalry in the late campaigns in the north of India, as soon as he saw the Fiorin, was struck with its exact resemblance to the Indian grass, and was satisfied they were of the same species. The characteristic mark of the Dub, according to Colonel Macan, is this, that from each joint a root strikes downwards, and a sprout shoots upwards. It is propagated in India, not by seed, but by scattering its strings on the surface, and dibbling them in. In the rainy season it creeps along the ground, and runs to a considerable length, rooting at every joint; in the dry season it is much covered by the dust and flying sand, whence it derives its name, which, in the Persian language, signifies "hidden." Colonel Macan adds, that it is most industriously sought for, and preferred to all other grasses in India, on account of its superiorly nutritive quality, as food for cattle.

In Sir W. Jones's catalogue of Indian plants, the Dub is classed as a species of Agrostis; and the engraving of it, which is copied from Dr. Roxburgh, represents it as a knotted or jointed grass, with fibres issuing from the lower, and sprouts from the upper side of each joint; but the panicle, or flowering part, is very different from that of the Fiorin, and resembles that of the Panicum dactylon, or creeping Panick grass; excepting that the spikes, which are there four in number, spread horizontally from the stalk. We shall take the liberty of extracting from Sir W. Jones's Botanical Observations on select Indian Plants, contained in the second volume of his works, the following account of the Durva or Dub. "Nothing essential can be added to the mere botanical description of this most beautiful grass, which Van Rheede has exhibited in a coarse delineation of its leaves only. Its flowers, in their perfect state, are among the loveliest objects in the vegetable world; and appear, through a lens, like minute rubies and emeralds, in constant motion from the least breath of air. It is the sweetest and most nutritious pasture for cattle; and its usefulness, added to its beauty, induced the Hindoos, in their earliest ages, to believe that it was the mansion of a benevolent nymph." Even the Veda celebrates it; as in the following text of the A't'harvana: "May Durva, which rose from the water of life, which has a hundred roots and a hundred stems, efface a hundred of my sins, and prolong my existence on earth a hundred years!"

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* The panicle, or flowering part of the Fiorin, judging from a drawing of it which accompanies Dr. Richardson's first memoir, resembles that of the festuca pratensis, or meadow fescue grass.

But the excellence of the Fiorin, supposing it to be the Agrostis stolonifera, is neither unknown nor uncelebrated in the annals of English agriculture; although, from particular circumstances, its history has been hitherto involved in much obscurity. It constitutes a considerable portion of the produce of a meadow in Wiltshire, the uncommon fertility of which was noticed by herbarists more than one hundred and fifty years since. This meadow, which is situated near Orcheston, about twelve miles to the north of Salisbury, is spoken of in Howe's Phytologia Britannica, which was published in the year 1650; and in Merret's Pinax, published in 1667. And references are made to these authors respecting it, in Bishop Gibson's additions to Camden. It is again mentioned in Stillingfleet's Miscellaneous Tracts. But no public inquiry took place respecting it, till some years ago: the Bath Agricultural Society, struck by the accounts of its remarkable fertility, employed agents for the purpose of ascertaining the nature of its produce. Since that time it has been visited by several botanists, from whose accounts we have collected those circumstances of its history, which are most applicable to the present occasion. The meadow is situated in the lowest part of a very narrow, winding valley, sheltered on each side by gradual but by no means lofty, acclivities of chalk. It is subject to frequent and continued inundations during the winter, and is rarely otherwise than swampy throughout the year, being often submerged by the water of a spring, which rises at about the distance of half a mile. It has been constantly observed, that the earlier the spring swells, the more plentiful is the crop. The immediate soil of the meadow consists of a bed of small, loose pebbles, which are all of a silicious nature, with a scanty covering of mould; and though the herbage of the adjoining meadows is altogether very exuberant, yet this exuberance may be traced, increasing or declining, according as the soil varies, more or less, from that of the principal meadow. The produce of the meadow consists of several grasses; the chief of which are varieties of the Poa trivialis, the Alopecurus pratensis, and the Agrostis stolonifera. It is mowed twice in summer, and, after a favourable season for watering, the first crop is nearly five tons from each acre; the second, about half as much. The first crop consists principally of the Poa trivialis; the last, of the Agrostis stolonifera. With respect to the grass of this celebrated meadow, it is observed, that all cattle eat it eagerly, and that horses will eat the hay made from it in preference to corn, mixed with chaff.

We have carried the foregoing observations, on the Indian and the Orcheston grass, further than to many may seem necessary; hoping they may help to elucidate the subject of the present memoir, of which we shall now give as short and connected an epitome as we are able.

The testimonies in favour of the excellent pasturage of Ireland are numerous, from Giraldus Cambrensis down to the present day. That which is most to our purpose we found in a letter, dated 1693, contained in a Natural History of Ireland, which was published at Dublin in 1726. This letter, in giving an account of the Giant's Causeway, and describing the neighbouring coast as elevated very far above the sea, but rising gradually on the land side, to the edge of the precipice, says, "that it is all covered with excellent sweet grass." It was in this very neighbourhood that Dr. Richardson first became acquainted with the Fiorin, in consequence of having purchased a small farm on the little peninsula of Portrush, which is situated a few miles to the south-west of the Giant's Causeway, and projects in the form of a cliff about half a mile into the Northern ocean. This farm, Dr. R. says, has long been famous for the verdure, abundance, and excellence of its pasture; and it has been repeatedly observed, that the tallow, and the butter made from the milk of the cattle fed there, surpassed, both in quantity and quality, those of any other farm in the country. The grass of this pasture consists almost entirely of Fiorin. During three and twenty years, Dr. R. made comparative experiments on the excellence of the Portrush pasturage, and that of some glebe which he possesses in the county of Tyrone; and though he had always good grass on the latter, and the glebe itself was in a very rich country, yet he invariably observed, that the same cow gave above a third more milk, and of a far superior quality, when fed on the Portrush, than on the Tyrone pasturage. This, he says, is the more remarkable, because the greater part of the Portrush meadow is composed of a very shallow soil, rarely three inches deep, covering a solid basaltick rock; and much burnt up in summer. In like manner, the Fiorin is distinguished by its high verdure on the cliffs and steeps facing the Northern ocean, particularly about the Giant's Causeway; occasionally forcing its roots into the crevices of the rock, and even into the diminutive intervals between the pillars of the causeway.

The present occasion does not require a minute statement of the observations and experiments made on this grass by Dr. Richardson. And, indeed, since he himself is " almost afraid of entering into a detail of its extraordinary qualities, entertaining faint hopes of obtaining credit or even attention, our readers will not be surprised if we make our selection with great caution; nor must he be offended with us if we doubt the reasonableness of those expectations, in which, too incautiously perhaps, for his future fame, he indulges. Thus, when he describes the Fiorin, not only as superior to most, if not all other grasses, and better fitted to every separate use to which grass can be applied; thriving almost equally in soils of the most contrary descriptions; the richest, the poorest, the deepest, and the shallowest, the tops of mountains, and the bottoms of valleys; bearing greater extremes of wet and of drought than any other grass, or, perhaps, vegetable; growing with full vigour under the shade of trees, and equally grateful to cattle when mowed from this situation, as from the open field; and, lastly, as being perfectly insensible to the highest degree of cold, since he saw the vegetation of its tenderest shoots uninterrupted by one of the bitterest frosts he remembers, and their lively green preserved equally, whether they were above the surface or buried under the snow; when, we say, he describes all these extraordinary and opposite qualities as existing in his favourite grass, who can choose but smile at his fond partiality? On the report of his experiments, we are fully disposed to rely with confidence; though even here we dare not anticipate the same degree of success, from the general cultivation of this grass, which he met with in the particular instances mentioned by him. The extent of that success may be judged of by the following statement.

In November, 1806, Dr. Richardson planted a piece of ground with Fiorin; of which, having obtained a number of distinct plants, he commenced by laying one down, and slightly covering the root with earth: he then stretched its string in a line, laying a little loose earth upon it here and there, merely for the purpose of holding it down. Where the string ended, another root was laid down, and its string was stretched in continuation of the former line, and so on to the end of the piece of ground. At two feet distance he made a similar row, parallel to the former; and thus continued till the whole piece of ground was planted. The strings soon showed symptoms of vegetation; and in the following July, the intermediate spaces were so completely occupied by new strings, that it was difficult to find out the original drills. The succeeding autumn was wet and severe, and the grass was, in consequence, flattened down; but, though matted like a crop of vetches, the under part was very thick, and exclusively composed of long strings, every one of which was in high vegetation, from the root to the extreme point.

A portion of this meadow was mowed, on December 7, 1807, and, contrary to Dr. Richardson's expectations, after so wet and severe a season, the sward, instead of sinking, was so raised up by the length and coarseness of the strings, that in half an hour it was dry. It was then made up in small heaps, which were afterwards turned over every other day, in order to expose the damp side to the wind. At the end of eight days these heaps were opened for half an hour, and then made into larger heaps, four feet high each ; these were opened three or four times during a fortnight, and were housed at the end of three weeks ; reckoning from the time when the grass was cut, during which the weather was singularly unfavourable, attended with great deluges of rain, succeeded by an extraordinary heavy fall of snow, which was followed by close damps.

Another portion of the same meadow was mowed on December 26 ; and the process of making the hay was conducted in the same manner as in the preceding instance : but, instead of being housed at the end of three weeks, it was suffered to remain under the open air for more than two months ; and on the 4th of march, it was still fresh and fragrant, and retained the healthy green in its strings ; and through the whole of the winter, there was not a single string that showed the least tendency to rot or decay.

Of the first crop, which was housed on December 28, several strings were set in a hot-house on the same day : these soon began to put forth fresh sprouts. Other strings, taken from the same hay, were planted on the 18th of January, and the 5th of February following. These, also, soon began to vegetate from every point. The same experiment was repeated on February 27, March 18, and April 8, on strings taken both from the hay that was housed, and from that which remained in the field ; and the success was the same in every instance.

This retentive faculty of the principle of vegetable life, so conspicuous in the Fiorin, Dr. Richardson thinks may be explained by its peculiar nature in not producing panicles till the second year; for, he argues, that as all vegetables appear to advance in a state of progressive improvement, until they arrive at the period of flowering and producing their seed, after which the powers of vegetation seem to abate; and as most grasses put forth their seed in the same year in which they were sown, it hence happens, that grasses in general will not support the inclemency of the succeeding winter; but the Fiorin not putting forth its panicles till the second year, and, consequently, not having attained its point of perfection till that time, the strings improve progressively through the whole of the first year; whence it follows, that it is even advantageous to defer the mowing of Fiorin till winter.

Another great advantage attending the cultivation of Fiorin is this, that whereas grass seed cannot be sown with prudence earlier than the middle of March, or later than the middle of September, at which seasons the farmer is necessarily very much engaged in other employments, the Fiorin strings may be planted at any time: and, according to Dr. Richardson, a crop may be obtained from this grass more cheaply and more expeditiously than from any other.

But it is unnecessary to dwell longer on the excellencies of this grass. Enough has been said, we conceive, to direct the attention of the agricultural reader to a subject, which, unless the author of the present memoir has greatly deceived himself, must be considered of the highest importance.

Quarterly Review.

Dr. New's Opinion respecting Sulphur and Charcoal.

The perusal of the Report by Messrs. Fourcroy, Deyeux, and Vanquelin, on a memoir of M. Bertholet, jun. entitled, "Inquiries concerning the reciprocal Action of Sulphur and Charcoal," has induced Dr. John New to publish an opinion, which he has for some years entertained,—that charcoal and hydrogen are modifications of one and the same substance, or that hydrogen is the base of charcoal. Should this opinion, the result of various experiments and observations, be confirmed, an important and extensive field will be opened to the scientific world. The pabulum of plants, and the origin of that immense quantity of carbonaceous matter, annually produced in the vegetable kingdom, would thus easily and satisfactorily be accounted for, as originating from water alone.

Medical and Physical Journal.

Account of a compensating Pendulum, by Lieutenant H. Kater.

The principal novelty in Lieut. Kater's pendulum is, that, instead of the various expansions of different metals being made to compensate each other, as in other pendulums of this description, in it the expansion of a deal rod is compensated by that of a zinc tube or frame, about ten inches long.

Two methods of applying the zinc are described: In the first it is cast into the form of a frame, ten inches high, two inches broad at the bottom, and one inch at the top, which is screwed to the cock of the time-piece in a verticle position.

In the top of this frame, to regulate its length, a thick screw of zinc, with a fine spiral, is inserted vertically, through a brass plate half an inch thick, soldered to the frame, and tapped to receive it, which has about a quarter of an inch of its top filled square, to admit an instrument being applied to turn it: this zinc screw has a hole drilled through it lengthways, through which a steel wire screw passes freely from the suspending spring of the pendulum, and is secured by a moveable nut resting on the top of the zinc screw. The suspending spring passes through a slit in a brass plate on which the zinc frame rests, and through a hole in the bottom of this frame, and is fastened to a slit in the lower end of the steel wire by a transverse pin. The steel wire screw regulates the length of the pendulum, as the zinc screw does that of the zinc compensation; a thin nut of brass is added to this latter screw, above the zinc frame, to fix it more steadily at the desired height.

In the second method of applying the zinc compensation, a square tube of zinc, seven inches long, three quarters of an inch in diameter outside, and four-tenths of an inch inside, surrounds the lower end of the deal rod of the pendulum, to the extremity of which it is connected by a zinc screw and steel screw, for the same purposes of regulation as in the first method, but in a reversed position. The zinc screw has a small octagonal plate of brass soldered at its lower end, to prevent injury from the nut of the steel screw on which it rests, and to form a sort of index for dividing each revolution of the screw, and of the nut, into eight parts.

The zinc tube, which rises upwards from the nut, passes freely into the center of the weight or bob of the pendulum, through a hole made so as to let it slide as it expands, but close enough to prevent a shake; at its upper end it is fastended to the center of the bob by means of two grooves cut in its sides; corresponding to which two similar grooves are made in the sides of the perforation of the bob, into the cavity formed by which melted lead is poured, after which the external plates of the bob are fastened together.

The pendulum rod for both methods is made of white deal, three quarters of an inch broad, and four-tenths of an inch thick, well seasoned, straight grained, and perfectly free from knots, baked in an oven till its outward surface became a little charred, then cleaned and coated several times with copal varnish, and having its ends dipped in melted sealing-wax, to exclude all moisture, which it is of the greatest importance to the perfection of the rod to repel perfectly. A cut is made at the top of the rod by a fine saw, to receive the end of the suspending spring, where it is secured by two or three small pins passing through both. When the second mode of applying the zinc is used, the lower end of the deal rod is cut at the sides so as to slide freely in the zinc tube, and at its extremity a brass cap is fastened, from the bottom of which proceeds the deal screw before described, by which the zinc tube and the bob are sustained.

The deal rod was found by experiments with a pyrometer to expand in one foot 0.0049 parts of an inch with 180° difference of temperature; the expansion of zinc Mr. Kater computes to be to that of deal as 353 to 49, and to that of steel as 353 to 147. And reckoning the steel screw to be 12.5 inches long, the steel suspending spring 3 inches, and the deal rod 37 inches long, in the first method, he calculates that the proper length of the zinc to compensate the expansions of these several parts should be 10.3 inches: and in the second method, reckoning the part of the steel screw included between the nut and the end of the rod to be two inches long, the suspending steel spring to be one inch long, and the deal rod to be 44.5 inches long, he computes that the zinc compensation should be 7.42 inches long.

Mr. Kater attaches a small weight below the bob, where he wishes to regulate the pendulum to the greatest nicety in the second method; which he thinks would be superior to the first, in being more simple and having the compensation shorter, if the zinc tube in it was not excluded from the free contact of the atmosphere, by being included in the bob of the pendulum.

The experiments of Lieut. Kater form a valuable addition to those before made on the same subject. The objection to his second method of applying the zinc compensation might perhaps be obviated by leaving an open passage for the air in the lead of the bob round the zinc tube, and preserving the steadiness of its motion by blunt points, or other small smooth surfaces, or by small friction wheels, projecting from the sides of the aperture.

It would tend much to complete information on this subject, if the earthen-ware rods for pendulums, proposed by Mr. Troughton, were tried in the same accurate manner in which the deal rods have been in the recited experiments. The principle on which Mr. Troughton recommended rods of pottery was, the permanent contraction observed in this substance by Mr.

Wedgewood at a given temperature. Rods of pottery would have the advantage of rods of deal in not being liable to be affected by damp. *Athenæum.*

Case of Suppression from a Metallic Bougie broken in the Urethra.

A case has occurred in St. Thomas's Hospital, the third which has come to our knowledge, of a hollow metallic bougie breaking in the urethra. The man had worn the bougie for some time, until the urine had so far acted upon it, as to destroy its cohesion. The man in attempting to extract it, left half the instrument in the bladder and urethra. An incision was obliged to be made into the urethra in perineo, and from thence the broken instrument was extracted. *London Medical Review.*

Case of Subclavian Aneurism.

In a case of subclavian aneurism which lately occurred in Guy's Hospital, Mr. A. Cooper attempted to tie the subclavian artery above the clavicle. The aneurism was very large, and the clavicle was thrust upward by the tumour, so as to make it impossible to pass a ligature under the artery without incurring the risk of including some of the nerves of the axillary plexus. The attempt was therefore abandoned. This artery has been successfully tied *below* the clavicle by Mr. Keate, but never yet as far as we know, *above* that bone. *Ibid.*

Operation for an Aneurism by Anastomosis in the Orbit of the Eye.

We understand that Mr. B. Travers, Demonstrator of Anatomy at Guy's Hospital, has tied the carotid artery in a woman, who laboured under an aneurism by anastomosis in the left orbit, by which the eye had been protruded from its socket. Two small round ligatures were applied, but the artery was not divided—the threads separated on the 21st and 22d day without hemorrhage, or any alteration in the functions of the brain. What will be the result of this operation upon the disease, cannot yet be precisely determined; but the effect of the ligature upon the artery, so far as respects the brain, was uninjurious, and coincides with that given by Mr. Cooper in his account of the case of carotid aneurism, in which he operated successfully. *Ibid.*

Two Operations for Injuries of the Head.

Two cases have lately occurred of fracture of the skull with depression, and without any symptoms in the first instance of injured brain. In the first, the operation was deferred until symptoms of inflammation of the brain were produced, when the bone was elevated by the trephine. But the symptoms were not lessened by the operation, and the boy died. In the second, the operation was performed several days after the accident, but before the symptoms of injured brain occurred, and he did well. The inference is obvious. In both cases there was a wound through the scalp to the bone. *Ibid.*

Account of Vaccination at Brazil.

Accounts from Brazil state, that the vaccine inoculation, first practised in St. Salvador, towards the close of 1804, has since been spread through all the provinces, by the orders of the Prince Regent. His Royal Highness appointed Dr. J. A. Barbosa to superintend and promote the new practice; and so beneficial have been its effects, that the small-pox, formerly very destructive there, has totally disappeared. *Med. & Phys. Journal.*

Appearance of a new Species of Wasp in various parts of England.

A species of wasp, which builds its nests in trees, has lately been observed in various parts of this country, and was frequently met with during the last summer in different parts of the West Riding of Yorkshire. It appears to be a new introduction, and is supposed to have been brought across the Atlantic into some of the ports on the western shore of the island, and is gradually spreading itself through the country. The trees on which the nests have been most frequently observed, are the gooseberry and currant, and an instance of it has been met with on the common elder, to which insects in general are averse. This species is smaller than the common wasp, but it is much less voracious, and less easily irritated. *Ibid.*

Description of Sir W. Clarges's new Life Boat.

Sir W. Clarges, Bart. has constructed a life boat on an improved principle, the leading features of which are, that she will not upset, sink, or be water logged; that she affords cabin room, and is like a man of war's launch, well built for rowing, the oars not on a curve, but nearly in a right line and low to the water, of which she draws little. The description of this boat is as follows:—Her length is thirty feet, her breadth ten, her depth three feet six inches. The space between her timbers is fitted up with pine wood; this is done with a view to prevent the water lodging there: the pine wood is well caulked and paid; she is buoyed up by eight metal cases, four on each side; these are water tight, and independent of each other. They will serve to buoy up six tons; but all the buoyant parts of the boat, taken collectively, will buoy up ten tons. The cases are securely decked over, and boarded at the sides with pine; there is a scuttle to each case, to put goods in; the edges are lined with baize; and over each scuttle, in the case, is one of wood of a larger size, the margin of which is lined in the same manner to exclude the water; between the cases are Norwegian balks, bolted to the bottom, fastened to each other by iron clamps, and decked over. The depth of her keel is nine inches below the starboard streak, the dead rising is four inches; her keel is narrow at the under part, and wide above, for the purpose of giving the timber a good bed, which will support the bolts, in case any necessity should arise, to encounter sand banks. In sailing over a bar, or in places where the water is shallow, the rudder will, with ease, draw up even with the keel, and when in deep water, it will let down easily, and with equal facility. The forecastle of the boat forms a cabin ten feet wide, six feet long, and four feet deep, into which women, children, and disabled persons may be put; it is amply supplied with air by means of two copper ventilators; it is furnished besides with two grapnels, very proper to be thrown out on board a wreck, to ride by; the grapnel ropes will assist the sufferers to remove and escape from the wreck to the boat. She is likewise equipped with masts and sails, and is as manageable with them as any boat of her dimensions can possibly be; in a tempest, however, she must be dismasted, and rowed by fourteen men with oars sixteen feet long, double banked; the men are all fastened to the thwarts by ropes, and cannot be washed from their seats. In his observations on this boat, Sir William says, "Having stated the leading features of my boat, I need not dwell on a few secondary points, which, however, it would be improper not to mention: these are, her being provided with small ropes or lines fastened to hooks on the gunwale, each having a piece of painted cork at the extremity, intended not only for persons who fall overboard or swim from a wreck, to see and catch hold of, but to tow any one for whom there may not be room in the boat; and her having a very powerful rudder. The copper cases, though affording additional security to those who choose to go to the expense, are no more a necessary point of my plan, than coppering her bottom. The wood work, if well executed and properly attended to, may be kept quite air tight. If the assistance of cork were to be called in, it appears to me that it might be better applied than in the other boats, by filling the cases with cork jackets, to take to a crowded wreck; in going off to which the cases would not be wanted for any other purpose; the jackets would not be an incumbrance. Every one must be aware of the use of the side cabins or cases, for stowing valuable goods from a richly laden vessel. A boat of this kind, but somewhat smaller dimensions, would be exceedingly useful to ships on discovery, and indeed to any large vessels; as it would not only answer for wooding and watering, but is peculiarly adapted for excursions up rivers or small inlets of the sea, or exploring clusters of islands. As a pleasure boat she answers extremely well; and with respect to her safety, I can say that I have sailed in her from Brighton, round the Cornish coast to Conway, in North Wales, without any accident, though we experienced some very dreadful weather on the voyage. *Ibid.*

Mr. Hume's new Test for detecting Arsenic.

Mr. Joseph Hume has discovered a new method of detecting arsenic. The test which he proposes as a substitute for those hitherto used, appears to be more efficacious, inasmuch as it produces a more copious precipitate from a given quantity of that substance. It is composed in the following manner: Let one grain of white oxide of arsenic, and the same quantity of carbonate of soda, be dissolved by boiling in ten or twelve ounces of distilled water, which ought to be done in a glass vessel; to this let a small quantity of the nitrate of silver be added, and a bright yellow precipitate will instantly appear. *Ibid.*

We have received from Paris and London several new publications, but they came to hand too late to be attended to in our present number. In the next, which will be published on the first of August, we shall give our readers an abstract of their contents.

GENERAL
RETURN OF DEATHS
IN THE
CITY OF NEW-YORK,
FOR THE
Years 1805, 1806, 1807, 1808, and 1809.

		Men.	Women.	Boys.	Girls.	Total
JANUARY..........	1805	57	55	37	29	178
	1806	63	23	34	26	146
	1807	44	37	43	34	158
	1808	51	36	39	36	162
	1809	45	33	52	33	163
FEBRUARY........	1805	47	41	36	22	146
	1806	41	43	27	23	134
	1807	70	27	41	28	166
	1808	58	41	40	33	172
	1809	56	37	32	38	163
MARCH............	1805	46	30	26	27	129
	1806	47	40	37	44	168
	1807	59	37	40	44	180
	1808	74	49	63	38	224
	1809	48	48	43	25	164
APRIL................	1805	71	44	38	26	179
	1806	70	48	29	40	187
	1807	75	64	36	40	215
	1808	66	38	34	39	177
	1809	42	49	28	27	146
MAY..................	1805	37	33	31	22	123
	1806	61	40	44	30	175
	1807	53	41	26	25	145
	1808	43	23	34	37	137
	1809	72	35	42	34	183
JUNE..................	1805	33	29	32	33	127
	1806	47	34	41	26	148
	1807	50	35	20	22	127
	1808	42	43	26	20	131
	1809	57	30	27	27	141
JULY..................	1805	49	44	63	46	202
	1806	53	45	55	49	212
	1807	56	40	59	40	195
	1808	52	30	49	48	179
	1809	48	36	43	43	170

		Men.	Women.	Boys.	Girls.	Total
AUGUST............	1805	52	54	115	98	319
	1806	50	38	92	75	255
	1807	86	71	81	90	328
	1808	43	29	53	59	184
	1809	53	38	97	89	277
SEPTEMBER......	1805	116	82	51	47	296
	1806	73	47	59	63	242
	1807	81	51	79	75	286
	1808	51	29	57	43	180
	1809	57	33	36	37	163
OCTOBER..........	1805	128	104	56	61	349
	1806	56	52	39	30	177
	1807	50	43	40	39	172
	1808	46	32	30	25	133
	1809	54	33	41	38	166
NOVEMBER.......	1805	51	42	28	19	140
	1806	58	50	42	31	181
	1807	54	40	25	39	158
	1808	40	39	34	38	151
	1809	78	50	46	31	205
DECEMBER.......	1805	61	50	31	22	164
	1806	63	47	56	34	200
	1807	63	43	30	46	182
	1808	52	48	47	37	184
	1809	63	42	34	28	167

AGES.	1805	1806	1807	1808	1809
Of the age of 1 year and under	503	522	537	435	465
Between the age of 1 and 2 years	210	242	208	224	207
2 5 do	157	190	176	161	165
5 10 do	74	75	78	93	88
10 20 do	130	111	111	119	120
20 30 do	351	250	295	244	254
30 40 do	336	284	319	271	279
40 50 do	239	223	218	187	192
50 60 do	155	122	165	114	128
60 70 do	102	102	97	75	103
70 80 do	61	62	68	64	66
80 90 do	28	34	34	24	36
90 100 do	5	8	5	3	4
100 and upwards do	1	0	1	0	1
Total	2352	2225	2312	2014	2108

*DISEASES.**	1804	1805	1806	1807	1808	1809
ABSCESS - - -	7	5	3	7	6	8
Aneurism - - -	—	—	—	1	—	1
Apoplexy - - -	22	31	20	38	23	20
Asthma - - -	6	3	11	3	5	4
Burned or Scalded - -	11	14	18	6	12	13
Cancer - - - -	7	6	10	5	4	6
Carbuncle - - -	—	—	—	—	1	—
Caries - - - -	—	—	—	—	1	1
Casualties - - -	24	21	24	27	24	34
Childbed - - - -	16	12	14	11	20	17
Chlorosis . - -	1	—	—	—	—	—
Cholera Morbus - - -	9	19	9	8	7	5
Cholic - - - -	5	—	4	9	—	7
Chorea - - - -	—	—	—	—	1	—
Cold - - - -	18	15	25	10	14	22
Consumption - - -	499	462	354	464	429	413
Convulsions - - -	169	192	195	219	159	163
Cramp in the Stomach -	3	1	1	4	5	4
Debility - - -	37	49	62	59	43	35
Decay - - - -	43	62	140	112	89	64
Diabetes - - -	—	1	—	1	—	—
Diarrhœa - - - -	13	16	1	7	3	26
Drinking cold water -	—	11	—	1	1	—
Dropsy - - - -	68	83	89	98	91	79

* During the year 1804 the bills of mortality were not kept with the same accuracy as at present. Hence, for that year, we are not able to discriminate the sexes, nor to ascertain the ages of the deceased. The total number of deaths, and the diseases of which they died, are the only particulars for which there are any authentic documents. And even this last will be found greatly defective, in comparison of the last five years; as, during the year 1804, there will be found at the end of the list of diseases, the following words, "*diseases not mentioned,*" 86, whereas, since that period, care has been taken to ascertain, with as much accuracy as possible, the real disease of which each individual died.

DISEASES.	1804	1805	1806	1807	1808	1809
Dropsy in the Head	19	16	22	30	28	28
Drowned	24	32	35	31	42	45
Dysentery	46	60	52	30	24	17
Dyspepsia, or Indigestion	—	—	—	1	—	3
Epilepsy	5	7	16	8	—	3
Fever, Bilious	8	22	11	9	3	9
Hectic	—	2	3	5	3	3
Inflammatory	1	9	6	3	1	6
Intermittent	5	4	7	3	5	12
Malignant	—	270	—	—	—	10
Puerperal	—	5	1	1	3	7
Putrid	6	3	3	2	3	3
Remittent	9	—	9	3	10	7
Scarlet	14	—	4	2	4	9
Typhus	47	75	93	106	62	73
Flux, Infantile	27	195	166	162	120	138
Fracture	—	—	—	4	2	7
Frost Bitten	—	4	2	2	—	2
Gout	—	2	3	1	—	—
Gravel	—	—	—	—	—	4
Hæmoptysis, or spitting blood	1	4	3	4	—	1
Hæmorrhage	4	—	3	1	—	3
Hanged	—	—	2	—	—	—
Hernia, or Rupture	—	—	—	—	3	1
Hives	75	70	106	101	94	102
Jaundice	3	4	4	9	13	5
Iliac Passion	—	—	—	—	—	1
Infanticide	1	4	2	1	5	4
Inflammation of the Bladder	1	—	—	1	—	1
Bowels	27	17	34	29	18	32
Brain	8	17	16	10	17	18
Lungs	36	53	58	58	40	44
Stomach	12	6	14	14	11	12
Influenza	3	2	—	16	2	—
Insanity	3	5	7	8	7	7
Intemperance, or Intoxication	12	17	11	14	18	28
Killed or Murdered	2	3	4	1	1	1
Leprosy	—	—	—	1	—	—
Leucorrhœa	—	—	—	—	1	—
Liver Disease	5	7	11	18	3	7
Locked Jaw	7	6	7	2	4	4
Manslaughter	—	—	—	1	—	—

DISEASES.	1804	1805	1806	1807	1808	1809
Measles - - - -	2	—	—	1	64	2
Menorrhagia - - -	—	—	—	3	—	—
Nervous Head-Ache - -	—	—	—	2	—	—
Old Age - - -	39	46	57	51	49	62
Palsey - - - -	14	15	20	23	16	22
Pleurisy - - -	36	32	41	46	41	40
Quinsy - - - -	9	6	4	5	4	2
Rheumatism - - -	3	3	3	4	5	4
Rickets - - - -	—	—	—	—	1	—
Rupture of a Blood Vessel	4	6	7	4	6	4
St. Anthony's Fire - -	—	5	7	4	—	2
Scrofula, or King's Evil -	6	4	4	4	4	7
Scurvy - - - -	1	1	—	6	1	2
Small Pox - - -	169	62	48	29	62	66
Sore Throat - - -	10	8	17	17	13	11
Spasms - - -	—	—	—	2	2	1
Spina Bifida - - -	—	—	—	—	—	1
Sprue - - - - -	23	23	36	32	16	21
Still born - - - -	22	47	51	76	64	70
Stone - - - -	—	—	—	2	—	2
Stranguary . . .	—	—	—	—	—	2
Sudden death - - -	37	18	27	34	21	16
Suicide - - - -	10	26	15	16	8	16
Syphilis - - - - -	9	14	12	15	10	13
Teething - - - -	33	30	44	35	37	48
Thrush - - -	—	—	—	2	—	1
Vomica - - - -	3	—	—	3	—	—
Vomiting blood - -	—	1	1	—	—	—
Ulcer - - - - -	4	5	6	4	1	3
White swelling - -	1	—	—	—	—	3
Whooping cough - -	19	19	72	35	35	50
Worms - - -	27	34	45	60	43	43
Diseases not mentioned	86	—	—	—	—	—
Total	2125	2352	2225	2312	2014	2108

APPENDIX.

From the Medical Repository.

" An Examination of '*A Review of a Dissertation on the Mineral Waters of Saratoga, &c. by* Valentine Seaman, *M. D.* &c. *as published in the second number of the New-York Medical and Philosophical Journal and Review.* T. & J. Swords. 1809.' *By the Author of said Dissertation.*

" AFTER some preliminary remarks, the Reviewers assert that '*Dr. S. had drawn from memory, on his map, the shape of the conical covering of the Spring at Saratoga, which as there delineated is materially incorrect, and conveys an erroneous idea of the object; for, if we mistake not,*' continue they, '*the real covering of the Spring is so flattened, and the declivity of the cone so gentle, that persons easily walk on its sides, and stand around the opening.*'

" On this subject, the author of the Dissertation has nothing to remark, but that (so far from *drawing from memory*) the shape of the rock, both as respects the size at its base and its elevation, was taken from observation made upon the spot. In regard to its general appearance, as represented upon the map, those who have seen the rock, before it became hidden by the floor of the building, which is now erected over it, all agree as to its accuracy.

" The Reviewers observe, that '*Dr. S. is inclined to believe Dr. Vandervoort correct, in the opinion that the* waters of Ballston contain no more than equal their bulk of carbonic acid gas, *because neither Bergman nor Henry could cause water* (at their temperature) *to absorb more than that proportion. But it may be urged on the other side of the question, that perhaps the pressure to which the waters of these Springs are exposed in their confinement under the earth, may be ten or a hundred times greater than either Bergman or Henry could give them. Thomson expressly says,* '*that some of the ærated alkaline water prepared in Britain as a medicine, is said to contain about thrice its volume of carbonic acid gas.*' *And we have reason to believe the Ballstown Springs more highly impregnated with this air than any artificial water can well be. We wish Dr. S. had endeavoured to settle it by actual experiment; for it ought to be remembered that mere reasoning can never decide a matter of fact.*' To this it may be rejoined, that unless we do put *reasoning* out of the question, it is really difficult to conceive how the waters of these Springs, which have a free outlet, could be exposed to such immense pressure as is here suggested; or that the water in the open Springs is more highly impregnated with the carbonic acid than any artificial water can well be. In Saunders's account of the mineral waters of Europe, there is not a single one stated to contain as much as equal its bulk of carbonic acid gas, in its composition: that of the Pyrmont Spring, which appears to be the most highly impregnated, contains only 26. cubic inches of the gas to 28. 875 of the water.

" *He* (Dr. S.) *tries his own skill at analysis. But here too he appears to have failed. He dropped into the water, previously evaporated to one fourth of its quantity*, and filtered. *some oxalic acid, which produced no* CLOUDINESS *or* PRECIPITATION; *whence he concludes no* MURIATE *of* LIME *exists in it. But this experiment, supposing no error in it, proves too much; for it also proves that there was no lime in the water in any shape, which both Dr. S. and Dr. Vandervoort enumerate among its ingredients; and* BOILING ALONE COULD NOT HAVE EXTRICATED IT.' '*It seems therefore, that our author's experiment is no better than his reasoning, there being an evident fallacy in both.*' Dr. S. in turn will feel himself well satisfied if the correctness of his reasoning shall equal that of his experiment. Whether his deductions from it are fallacious or not, the experiment stands true. It was made (as were the other experiments on the water of Ballstown) in the presence of Dr. Walters and Dr. Cock, and before three of his pupils, viz. John Fitz Randolph, Wm. Golden, and John L. Fitz Randolph, all of whom could testify to its accuracy if required. This, however, is unnecessary, since the result of it was nothing more than what every *Chemist* would reasonably expect. For *Kirwan*, in his Treatise on Mineral Waters, observes, that " Aerated lime, argil and aerated iron are *entirely* precipitated by boiling a quarter of an hour." And again, " If the lime were held in solution by fixed air only, it would be precipitated by continued ebullition or partial evaporation, and, consequently, this test (oxalic acid) could precipitate none; but if it were dissolved by a mineral (fixed) acid, this test would cause a precipitate both before and after partial evaporation or boiling.'

" *Thomson* says, 'Earthy and metallic carbonates are precipitated by boiling the water containing them; except carbonate of magnesia, which is only precipitated imperfectly.'

" Henry, when speaking of the different states in which lime is held in solution in mineral waters, says, ' If the oxalic acid occasions a precipitate before, but not after, boiling, the lime is dissolved by an excess of carbonic acid: if after boiling, by a fixed acid.'

" The Reviewers again remark, '*Dr S. from the effects of reagents on the waters, concludes they contain* CARBONATE OF SODA; *and if so, he says it is impossible that the muriate of magnesia should exist in them, for the superior attraction of the alkali for the muriatic acid would utterly prevent the formation of a magnesian muriate. This argument, however, is not so conclusive as he seems to think. Although the soda may not be neutralized by the muriatic acid, yet small portions of that acid may at the same time be combined with the lime and magnesia.*' Thus much for the Reviewers' opinion of the argument. The author will not contend for the dust of the balance; he cannot, however, but observe that Henry, whose work is pronounced by Professor De Witt to be correct, seems to think a little differently from them: he says that ' Muriate of magnesia and alkaline carbonates cannot exist together in solution.'

" After several other minor observations, the Reviewers thus finally conclude. '*Notwithstanding the critical remarks we have been compelled to make on some parts of this pamphlet, we wish not to be understood as casting a censure on the work generally; we wish rather to encourage than to discountenance investigations of this kind; but we wish also to see them well executed. The author of the publication before us, certainly deserves great credit for the labour he has bestowed on this subject; and particularly for collecting information concerning the medical virtues of these waters. This information was much wanted, and will undoubtedly be useful. The whole performance is indeed well calculated to amuse and inform the passing traveller, who has not had an opportunity to see and taste for himself. This, we presume, was the principal design of the writer, and thus far his design will be accomplished. The man of science assuredly cannot drink deep of this pierian fount, nor will he find it either pungent to his taste or exhilarating to his mind; but, like the plain and wholesome waters of ordinary springs, it may quench the public thirst, though it do not brighten the public intellect.*'

" The writer of the Dissertation, with equal liberality would likewise observe, that, *notwithstanding the* little objections he *has been compelled to make to some parts of this* review, he *wishes not to be understood as casting a censure on* such works *generally, but he wishes* also *to see them well executed.* The criticisms *indeed seem well calculated to amuse the passing* reader *who has* *not had an opportunity* of knowing any thing of chemistry himself. *This* he *presumes was the principal design of the writers. The man of science assuredly*, unless he makes a large swallow, must here indeed have something *pungent to his taste*, if not exciting to his hilarity. Must not their great discovery of the power of water to hold the carbonate of lime in solution, in spite of the decomposing power of heat, *brighten the public intellect?* How gratifying it is to observe with what facility, by their own intuitive authority, they prostrate a Bergman, a Kirwan, a Murray, and a host of other drudging experimenters, at their feet! Those old-fashion-folks used to toil and dig deep for their facts, while these modern adepts, with unsoiled hands, seem to flourish about, upon the high ground of science, with all imaginable ease."

"*The remainder of Dr. Seaman's piece contains no additional objections of material import. It is moreover of a personal nature, and has for its object inquiries foreign to the subject. It is therefore omitted.*"

We insert the foregoing "*Examination*" of Dr. Seaman with cheerfulness, inasmuch as it relates to facts, and matters of science, proper for discussion; except indeed the last paragraph; which, as it only expresses the mortification of the author at the character given of his work, is frivolous and unworthy of notice. To this we subjoin the very full and satisfactory answer we have received from the writer of the Review. We have only to regret that on this occasion any personal observations should be mingled with scientific investigation. These Dr. S. has introduced into his publication in a very undignified and improper manner. And though we are persuaded the answer of the Reviewer is seasoned with too much severity for ordinary cases of literary disputation; yet in this instance the circumstances that have produced it, appear to justify the spirit with which it is written. The true course of Dr. S. would have been to have sent his remarks originally to us; and all of them which were proper, and pertinent to the occasion, should have been published in our Journal. The Reviewer then would no doubt have given them the most respectful consideration; and it would have been in our power to have prevented all personalities, and pointed animadversions on either side. But Dr. S. as if conscious that he was deviating from the path of propriety in his reply; and knowing that it contained unjustifiable insinuations, which could find no countenance here, did not send his performance to us, until after he had procured its insertion in the Medical Repository, the Editors of which, in humble subserviency to him, very readily committed this breach of decorum. Under these circumstances, we can neither exclude the Reviewer from publishing his answer, strong and "*pungent*" as it is; nor can we, without manifest injustice, restrict him in the utmost latitude of observation he may take, consistent with the rules of propriety. But here the discussion must stop in our Journal.

While we are on this subject, it is proper to mention generally, that whenever facts stated in a Review are controverted, and a reply is received, of sufficient importance to be noticed, in the estimation of the person who may have written that particular review, it will be promptly inserted in our publication. But in all matters of opinion with respect to the character or merit of a book, the judgment pronounced by the Reviewer must be final. No remarks can be admitted. If we should adopt the practice of countenancing such fruitless animadversions, our work might soon be filled with nonsense. The *opinion* of an author, or even his *reasoning*, in favour of the perfection of his own work, can have but little weight: and if his faults are pointed out, it will not avail him to vent his *feelings* to the world; for it seldom feels as he feels, or thinks as he thinks.

Our Reviews, we hope, will always be liberal, though they cannot always approve. To bestow indiscriminate praise and flattery on every book and author, as some of our brother Reviewers are in the habit of doing, is as great an imposition on the public, as indiscriminate censure would be on writers. This is sufficiently evinced by the little estimation in which such reviews are held, and by the trifling importance attached to the publications containing them. We cannot thus degrade our judgment, to flatter the foibles of any writer. When a man looks into a review, he wants not only a general account of the contents of the work under examination, but also a decided opinion concerning its character and value. Without this a review is of little use. It neither contains the necessary information required by the reader, nor can it operate as a corrective for the improvement of literature or science. In the prosecution of our work, while we shall endeavour to do inviolable justice to every author, we are determined not to neglect our duty to the public.

THE EDITORS.

To Valentine Seaman, M. D.

Sir,

You have published, in the Medical Repository, a two-fold reply to a review of your dissertation on the Saratoga and Ballstown waters in the Medical and Philosophical Journal. That part of this reply which is first in order, is decent and proper. It is such as you had a right to make, and if you had confined yourself to the manner and matter there adopted, you would have been entitled to a respectful answer. But the second part is rude, and reprehensible. It is pointed at individuals whom you were not authorized to address, and it is altogether irrelevant to the occasion. Having taken this improper course, your observations should have passed unnoticed by me, but for the three following considerations.

1st. You have misrepresented a material part of the review in question. On this point *justice to myself* requires me to give it such a minute and perfect explanation as to bring it, if possible, within the sphere of your comprehension.

2d. You have uttered mysterious insinuations, implicating the motives of the Reviewer, not from any thing "*manifested*" on the face of the review, "*but upon very different grounds.*" For this violation of decorum, *justice to you* demands that you should receive appropriate correction.

3d. You have persisted in repeating again and again the most flattering commendations on your *own* experiments, your *own* reasoning, and your *own* analysis, as patterns of correctness and truth, superior to those of other chemists, whom you at least ought not to consider beneath yourself. And this you have done without, at the same time, giving sufficient evidence, by your work itself, of its accuracy or its excellence. For this your *modest assurance*, *justice to the public* renders it necessary that your pamphlet should be more minutely examined than it has hitherto been, to expose its errors to every eye.

My business then is to administer distributive justice to the *public*, to *you*, and to *myself*. In order to do this, however, with sufficient force and effect, I find I must descend from the dignity of a Reviewer, and become your correspondent. I must lay aside for a moment my office of "*public censor*," and assume that of your antagonist. There are wrongs, you know, against which the *civil* code has provided no remedy. In that case the execution of natural law devolves on individuals: and there are transgressions in the republic of letters, not within the jurisdiction of a Reviewer's court. This is precisely your case. The chaste and delicate rules which govern there cannot reach it. For you there is no adequate retribution from that exalted judgment seat. The justice there denied, you shall receive at the hands of

THE REVIEWER.

Answer to Dr. Seaman's "Examination" of a Review, of his Dissertation on the Saratoga and Ballstown Waters, in the second number of this Journal, by the Writer of the Review.

In the year 1807, it appears an analysis was made of the Ballstown water, by an eminent French chemist. This was sent from Paris to Dr. Hosack of this city. By him it was first published in a newspaper, and afterwards it was inserted in the Medical Repository of New-York, and the Monthly Anthology of Boston. That we may be able to compare it with Dr. Seaman's account, I shall introduce it here. The public will no doubt be gratified by perusing it. The original is in the words following, to wit:

"Mr. Lewis,

"Presuming that an accurate knowledge of the composition of the Ballstown Waters may be of public benefit, and lead to a more general use of this valuable article of the Materia Medica, I request the favour of you to give a place in your paper to the following analysis, which has been made in France by one of her most celebrated chemists.

"It may be proper to remark, that the water was carefully sealed at the spring, and conveyed by a gentleman who had been in the habit of drinking it. He observes, 'On my arrival at Paris, I drank two bottles of the water, and found no difference in the taste or effect from that experienced last year at Ballstown, from which I conclude that it suffers little or no alteration from transportation.'

"Analysis of a bottle of Ballstown water, containing twenty-five ounces:

"1 Carbonic acid gas, or fixed air, three times its bulk.
"2 Muriate of soda or marine salt, 31 grains.
"3 Carbonate of lime supersaturated, 22 grains.
"4 Muriate of Magnesia, 12½ grains.
"5 Muriate of lime, 5 grains.
"6 Carbonate of iron, 4 grains.

"The chemist proceeds to add, 'no mineral water of our continent is so rich in saline substances of this sort. That of Vichy, which is in great repute, does not contain more than the *tenth of a grain* of the carbonate of iron to a bottle; while that of Ballstown contains *four grains:* And it is chiefly to the iron that these waters owe their tonic and deobstruent qualities. Another advantage of the American mineral water is, that by its gently cathartic operation, it is no less calculated to evacuate bile, than to give tone to the vascular system. On these accounts it is a valuable remedy in a great variety of diseases of debility, and appears to be formed by nature in the best possible proportions to give it efficacy.' He adds, 'I have no doubt, when known, it must become an important object of commerce.'

"Two inferences worthy of notice are to be deduced from the above analysis of this powerful chalybeate; that in some diseases, as in consumption of the lungs, it has hitherto been improperly employed; but that there are also many others, for which it has not been generally used, and in which it promises to be of great value.

"I am, Sir, yours,

"DAVID HOSACK."

If this analysis of "*a stale bottle*", of mineral water, as Dr. Seaman contemptuously calls it, had evaporated with the fugitive productions of the day, it might have contributed much to the tranquillity of his mind: But unfortunately for him it remained embodied in a visible form. It moved about on the breath of fame. It hovered around his imagination; and, like a ghost of the night, it followed his footsteps, and incessantly disturbed his philosophic repose. "Had it only *appeared*," says the Doctor,* "had it only *appeared* in one of our newspapers, it might, however incorrect, have passed unnoticed, as its errors would probably have died with the day; but when we see this same account afterwards treasured up in the more grave form of an article in the Medical Repository, and again republished in the Monthly Anthology of Boston, acknowledging it, at the same time, as a *public benefit*, we being *thereby* made acquainted with the composition of this valuable article of the Materia Medica, it certainly becomes a matter of animadversion: for who, upon reading that article, could believe that *our citizens*, who had for years been drinking of the waters of *Saratoga*, knew any thing of their qualities; or that *our physicians*, who were in the daily practice of prescribing them, had ever ascertained the nature of their contents? Whereas the truth is, that they have undergone a *thorough investigation*, and that in the large way, and that a *full account* of them has been published a number of years since." Now, if Dr. S. was seriously alarmed and offended, as he appears to have been, at the publication of the French analysis, I fear he has found no one to sympathize with him. I confess myself at a loss to imagine what mischief it could produce, unless indeed it made an inroad on Dr. Seaman's monopoly of science. If he felt conscious that his account of the Saratoga waters was correct, he could have nothing to fear from an *erroneous analysis* of the Ballstown water. If a *thorough investigation* of these waters had been made in the large way by *our physicians*, what could they apprehend from the counter analysis of the whole host of French chemists? Surely nothing. But yet Dr. S. appeared disturbed; and seemed to anticipate some great evil from a simple French analysis of the Ballstown water. The cause of this singular deportment was obscurely hinted at by himself. It evidently was a well formed presentiment, that we should "*thereby*" be made acquainted with the contents of this water, and thence derive a "*public benefit.*" The consequence of which would be, that his "dissertation" would be consigned to the "*Grotto del Cani,*" from whence it originated. The truth now appears to be, that instead of a *thorough investigation*, these waters had undergone only a *superficial* examination, and that instead of a *full account* being given, a very deficient one was published. Dr. S. himself acknowledges that since that *thorough investigation*, and probably also since the French analysis was published, he has discovered an additional material in the Saratoga water, viz. *carbonate of magnesia*, which he had positively stated in his first *full account*, not to be contained in the water at all. And to Dr. Vandervoort's *full account* of the Ballstown water, Dr. S. had added two materials, which had escaped the *thorough investigation* of that chemist. These things may perhaps in some measure account for that unaccountable thing of which Dr. Seaman complains so grievously, to wit, that no one seemed to know that *our physicians* knew the contents of these waters; and that their *thorough investigations* should remain unnoticed, while a contemptible, *erroneous*, and mischievous French analysis, by a French chemist, attracted some attention, and was even published in a newspaper. But this circumstance will be still more satisfactorily explained in the sequel of my observations.

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* Med. Repos. Hexade ii. vol. v. p. 253. 1807.

In the course of Dr. Seaman's first examination, in 1807, of the French analysis, in the Medical Repository, he gravely announces the important fact, to which he alluded in the quotation I have made, to wit, that in the year 1793 he had actually published a "*dissertation on the mineral waters of Saratoga,*" and had "given a particular analysis of the water from that *most admirably curious receptacle*, the Rock." He also mentions, that "two years afterwards, viz. in 1795, Dr. Peter Vandervoort gave an account of the analysis of the mineral waters of Ballstown, as made at the springs. That in this treatise the several experiments are detailed at large, and that from them he inferred, that one gallon contained 100.75 grains of *carbonic acid*, which is about equal to its bulk when separated in the form of gas ;

144 grains of the muriate of soda ;
133 grains of the carbonate of lime ;
155 grains of the carbonate of iron."

At the close of his remarks, Dr. S. gives the result of his own experiments as follows : " Finally, I think we may safely conclude that the mineral waters of Ballstown hold in solution

Carbonic acid,
Muriate of soda,
Carbonate of lime,
Carbonate of iron, and
Carbonate of magnesia.

Nor will any experiments yet published warrant us in concluding that they contain any thing else."

In deciding on the comparative merits of the three foregoing analyses, in this article of the Medical Repository, Dr. S. at once takes the highest seat in science, and passes judgment in favour of *himself:* and that he may appear the more pre-eminent above his rivals, he has invented a new species of grammatical comparison, by which he ascends at least two degrees at every step—it is *bad*, *good*, *best*. The French analysis he presumes altogether *bad*, inasmuch as it differs in some points from his, and in others from Dr. Vandervoort's. The analysis of Dr. V. is *good* where it differs from the French statement, though it does not come up to Dr. S.'s standard ; and his own must be *best*, inasmuch as it differs from both the others. This is the *pith and marrow* of his decision. He forms a kind of natural climax. The French chemist is *highly erroneous;* Dr. V. is correct so far as he goes ; and Dr. S. is transcendently perfect, having discovered a material that had *eluded the researches* of both the others. The French chemist Dr. S. prostrates at once in the dust, places Dr. V. standing on the Frenchman's back, and he, Dr. S. mounting on Dr. V.'s shoulders, caps the climax, and like the fly on the coach exclaims, "*behold what a dust I raise.*" Now, all this, it must be confessed, is very becoming and modest, in a grave and learned author like Dr. S. Moreover, a little self-sufficiency goes far towards being thought most wondrous wise ; and, to "*old fashioned folks*," is a much easier method of getting reputation than deep study.

In the second edition of Dr. S.'s dissertation he repeats the substance of what he had published in 1793, with respect to the Saratoga springs, and also of what he had introduced, in 1807, into the Medical Repository, concerning the French analysis of the Ballstown water. He has thought proper, however, to make some considerable variations on several points; and some of these will be noticed in my subsequent remarks. The Reviewer, in examining this last performance, without presuming, as Dr. S. had done, to decide which of these three analyses was most correct, ventured only to doubt Dr. Seaman's infallibility, and pointed out some circumstances wherein his objections to the French analysis might not be well founded. Dr. Seaman had never seen the experiments of the French chemist, and therefore it was impossible for him to judge of the accuracy of the *results*, (which alone were published) except by another series of accurate experiments made by himself. But the experiments, such as they are, of Dr. Seaman, were before us; and, on examination, there was reason to believe, that from some of them (to use the most delicate phrase which could justly be applied) *no certain conclusions could be drawn.* It was well known that Dr. S. when he made his first experiments on the Saratoga waters, in 1793. was young and unexperienced; and on referring to those experiments, I found many of them altogether destitute of the necessary precision and precaution required in such researches. From some of them indeed *no conclusions at all could be drawn*, as will presently appear. Since that period it was never understood that Dr. S. was either in the habit of experimenting, or even in the habit of reading the experiments of others engaged in making discoveries in chemical science. For years past the greatest part of his time has certainly been absorbed in the drudgery of practising medicine. Under these circumstances, it could hardly be supposed that he prepared his own tests, which therefore might be impure; or that he was so much conversant in practical chemistry as to observe, with great minuteness, the results that might be produced even by good tests, and judicious operations. Nor did he take the certain methods of ascertaining what he wished, particularly with respect to the presence of muriate of lime and magnesia, but pursued short and uncertain operations of his own. All these considerations were good grounds for not placing the most implicit confidence either in his *experiments* or his *reasoning*, by which he arrogated to himself the prerogative of settling the difference between two other chemists. After examining the fruitless manner in which he had proceeded with respect to the several points in question. the Reviewer ventured further, on the whole, to intimate that Dr. S. had not succeeded in accomplishing what he had assumed to do in a satisfactory manner. At the same time, all the credit was given him that could reasonably be wished or expected, for his industry in research, and for the valuable matters contained in his publication. Judge then reader, after accompanying me in the following more minute investigation, whether the Reviewer was not correct and judicious in his decision? Whether he was not mild and delicate in his manner? Whether he did not do his duty to the author and to the public? Dr. Seaman himself, if he does not admit both the propriety of the review, and the correctness of the motives that dictated it, will at least regret severely that he ever has questioned either of them.

In analysing the Saratoga waters, one of the first things was to ascertain the quantity of carbonic acid gas, or fixed air, contained in the water. How does Dr. S. accomplish this? He finds that Berghman has said, that water at the temperature of 55° is capable of absorbing about equal its bulk of cretaceous acid; and as the bubbles of this acid are continually passing through this water, Dr. S. is *clear* it must be *fully saturated*. But as the temperature of the spring is not so low as 55°, he is obliged to *conclude* the water contains not quite equal its bulk of fixed air. He then proceeds to ascertain, by experiment, what he had already discovered *clearly* and *conclusively* by reasoning. He collected from a given quantity of the spring water "*upwards of its bulk of ærial fluid in a vessel previously filled with, and inverted in hot water;*" the greater part of this he "*takes* to be the above mentioned acid; the rest *perhaps* partly atmospheric *mephitis*, and partly vital air." Now, nothing can be more vague and inaccurate than this experiment. Dr. Seaman certainly requires not to be told that he should have used mercury, for the purpose of collecting his air in a receiver, instead of *hot water*. But after operating in the way he has done, will he tell us, if water at 55° absorbs equal its bulk of fixed air, how much the *hot water* absorbed over which he collected it? How much space, if any, in the glass vessel was occupied by the vapour of that *hot water?* What was the temperature and consequent state of expansion of the air bubbling through that *hot water?* and what quantities of *vital air*, and *atmospheric mephitis* were mixed with the other air, which he retained by means of his *hot water?* Can any certain conclusions be drawn from such an experiment? Or rather, can any conculsions be drawn from it? Most certainly not, except by the incomprehensible sagacity of Dr. S. He infers from it that a quart of the water contains "*about* 40 *cubic inches*" of carbonic acid. But the capacity of a quart is 57¾ inches. Instead then of an equal bulk of this air, the water contains only a little more than two thirds of its bulk. What has become of the deficiency of 18 cubic inches or more to make " *upwards of its bulk;*" or, according to his former conclusion from Berghman, equal its bulk? Perhaps he knows by intuition, that these 18 inches were *oxigen* or *mephitis*, or some such thing. If so, he may perhaps also know by intuition the effect of his *hot water* on his *mephitis*. Truly, the value of this experiment, if *experiment* it can be called, is not equal to a *conjecture:* for Dr. S. most assuredly came nearer the truth by guessing, than by distilling.

In proceeding further in the analysis, Dr. S. according to his own account, treated the residuum procured by evaporation with *spirits of wine* containing so much water as to dissolve 17 grains of muriate of soda, or common salt, instead of using alkohol, which would have dissolved none. Now, if the water in his spirits of wine dissolved 17 grains of common salt, how much of the carbonate of soda contained in the residuum would it have dissolved? But he has answered this question already; for, by evaporation he obtained nothing but cubic crystals.* Although carbonate of soda is much more soluble than common salt, yet, wonderful to tell, in this instance the water in the spirits refused to melt it, for he found the whole 26 grains, which he supposes the spring water to contain, in the residuum, and not one grain in the watery spirits, which had, notwithstanding, taken up a considerable quantity of more insoluble muriate of soda. Yet, from this experiment, with the marks of inconsistency and error on the very face of it, in his usual manner of bold assurance, he *concludes positively* with respect to a fact; which, however, he says, he knew before he made the experiment. Dr. S. certainly has a remarkable faculty of accommodating the results of his experiments to his preconceived knowledge, and thus by a happy coincidence to establish both.

Again, in ascertaining the quantity of *carbonate of iron*,† Dr.

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[*] Dr. S. also says, that because he obtained nothing but *cubic crystals by evaporation*, there could be *no muriate of lime or muriate of magnesia* in the water. But he does not tell us to what *extent* he evaporated. Muriate of lime is soluble in about half its weight of water, and muriate of magnesia in about equal its weight. In alkohol, muriate of lime is soluble in about equal its weight; and one hundred parts of alkohol will dissolve 547 parts of muriate of magnesia. Unless, therefore, he evaporated nearly to dryness, he could not have expected any other crystals than those of common salt; and, if he evaporated to dryness, he would have had a confused mass, the composition of which he could not tell by inspection.

[†] Dr. S. found that after adding sulphuric acid to the water, it became blue by the Prussian test, but not before. In his first edition he says in a note, " This experiment, *which is an original one*, shows by what method we may detect ærated iron, by the Prussian lime water." But in his second edition he has wisely omitted this *original* discovery. A little reading, after 16 years, seems to have suppressed

S. exposed the last residuum of 204 grains to the sun for some time, to rust the iron, and make it *insoluble* in distilled vinegar, as he says in his first edition, but in the last it is, to make it *soluble.* This last probably is a typographical error; for highly oxidized iron is insoluble. And he collected on his filter 14 grains, which "being dissolved in vitriolic acid, afforded 8½ grains of iron, precipitated in form of Prussian blue by the Prussian lime water." But Prussian blue *is composed, according to the most accurate experiments hitherto made, of equal parts of oxid of iron and prussic acid.** This fact, to which Dr. S. has given no attention, reduces the oxid of iron in his precipitate to 4¼ grains. Moreover, the *Prussian lime water*, and Prussian alkali, used as tests, contain a very considerable quantity of iron; they are triple salts; the one is the prussiate of lime and iron, and the other a prussiate of potash and iron. This last contains usually "*from* 22 *to* 30 *per cent. and upwards of iron*,"† and it is presumed the first may contain about the same quantity. Dr. Seaman then ought to have ascertained the quantity of iron in the test he used, and deducted it from the weight of his *precipitate*, which would have reduced his oxid of iron contained in the water considerably less than 4¼ grains; and then, if to this last quantity he had added the additional weight of the carbonic acid necessary to constitute the carbonate of iron, he might have come near the truth. But as the experiment stands, for any thing that appears to the contrary, the whole of the 4¼ grains of iron might have been contained in the quantity of *Prussian lime water* he used; for if he employed only 15 grains, it probably contained about that weight of iron. Dr. Seaman, however, will not "*contend for the dust of the balance.*" He pours in his Prussian lime water without weight or measure, and if he obtains 8½ grains of Prussian blue, no matter whether it is derived from his test, or from his residuum; and though half its weight is prussic acid, yet having actually obtained 8½ grains of the *prussiate of iron*, he says therefore it appears the water contains 8½ grains of *carbonate of iron.* But it appears there was left 14 grains on his filter, insoluble in distilled vinegar. Now, Henry says, "*distilled vinegar takes up magnesia and lime, but leaves undissolved alumine and highly oxidized iron;*" and also silex. Dr. S. does not pretend that water contains either alumine or

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some of his *original* vanity, and he has at length *discovered* that he never made any discovery at all. I have already mentioned the author as an *original genius;* but he is so exceedingly modest that he will not now, it seems, give himself credit for any *originality.*

* Thomson's Chemistry.

† Henry's Chemistry, p. 320.

silex. The 14 grains then must have been highly oxidized iron; but this quantity of oxid of iron, instead of yielding only 8½ grains of Prussian blue, ought to have given 28 grains. Will Dr. Seaman tell us the cause of this deficiency in his result? No; the truth is, he can tell nothing about it. No conclusions can be drawn from his experiment. It can serve no other purpose than to show his presumption in misleading and imposing on the public. Dr. Vandervoort states the proportion of carbonate of iron in the Ballstown water to be double, and the French analysis makes it treble the quantity which Dr. S. supposed he had found in the Saratoga water; and there is no reason to believe that the Saratoga water contains less than the Ballstown. The French analysis notes particularly the unusual quantity of iron dissolved in these springs; and we are therefore to presume the chemist to have been attentive to this part of his analysis. If, therefore, confidence is to be placed in any of the accounts we have had of these waters, it must be in that from France. Dr. S.'s analysis is certainly not entitled to any.

After these specimens of the accuracy and dexterity of our renowned author, who tells the world that he had *thoroughly* investigated the subject; that he had given a *particular* analysis of the Saratoga waters in the large way; that he had published a very *full account* of them; and no doubt intended that it should be inferred from thence that all further investigation was useless, as every other account differing from his must necessarily be erroneous;—I say, after these specimens of his skill and knowledge, we need go no farther to be satisfied how great a chemist he must be, and of course how very diffident he is in telling the public that he really is so, notwithstanding they may not have penetration enough, at first sight, to discover the extraordinary minuteness of his research, or the depth of his science. There are some other facts, however apparent in his publications, which ought to be noticed.

1. In his second edition he says not a word about the *hot water* with which his vessel was filled, wherein he collected the carbonic acid gas. This important part of the experiment, stated in his first edition, is here entirely omitted; and the reader is left to infer, if he has any confidence in Dr. Seaman, that the experiment was made in the usual, and only proper way in which it could be made, by means of mercury: for it is not to be presumed that so learned an operator as our author pretends to be, would conduct any experiment in a manner at which a school-boy would smile; nor could it be believed that he would attempt to draw any inference from it, if it had been so made.

If, therefore, Dr. S. has not exhibited the most extraordinary correctness in his experiment, he has certainly shown great dexterity and address in presenting it to the public in so plausible a way, that no one can fail in being deceived by it. Surely this is an improvement in his new edition, for which he ought to receive all due credit and reputation.

2. In his first edition Dr. S. says of the Saratoga water, after being boiled, that "its appearance was *not altered* by the *caustic vegetable alkali*," which ought to have precipitated the magnesia if any had been present. But since the appearance of the French analysis, stating the existence of muriate of *magnesia* in the Ballstown water, Dr. S. in his second edition, precisely contradicts his former statement of this experiment in the first. He says of the water, "it formed a *light cloud* upon standing a little time with the *caustic vegetable alkali*." So also in his first edition he ascribes the phenomena indicating the presence of an alkali in the water, to an alkaline salt only, and not to magnesia. But in his second edition he says, "the phenomena were produced, at least *in part*, by the carbonate of an alkali, and not by the *carbonate of magnesia alone*." So again in his first edition he says the residuum procured by evaporation, formed with vitriolic acid a *gypsum*, but *no bitter salt;* and that hence it contained *no magnesia*. But in his second edition, although he says it formed *no bitter salt*, yet he admits it contains a *little magnesia*. From the same experiments then, stated differently at different times, our author concludes positively that the Saratoga waters contain magnesia, and no magnesia. I have before mentioned his happy faculty of accommodating the results of his experiments to his preconceived notions and views. The reader will judge whether this is an example of the exertion of that faculty, or whether it is to be considered as the result of some other power still more remarkable.

3. As Dr. Seaman had proved, by his experiments in 1793, that the *Saratoga* water contained neither magnesia, nor muriate of magnesia, and afterwards found himself under the necessity of proving that it did contain some magnesia; so, to avoid a similar dilemma, when he comes to examine the Ballstown water in 1807, in the Medical Repository, he at once takes to himself great merit for clearly proving it to contain a substance which seemed to have eluded the researches of both Dr. Vandervoort and the French chemist, viz. the *carbonate of magnesia;* and he admits also that it may contain *muriate of magnesia*, his experiments not authorizing him to a certainty to say that it did not contain that salt in its composition. The nine

experiments there mentioned, he states to prove the existence of *carbonate of magnesia*, and carbonate of magnesia *only*, for he says, " all these phenomena, as well as the discharge of air bubbles by the addition of oxalic acid, are easily explainable upon the presumption of the presence of *carbonate of magnesia;* and as they seem inexplicable upon any other principle, in my opinion they fully establish the fact of that substance being a part of the composition of these waters." These same experiments he has copied into his second edition in 1809, together with an additional one with turmerick; and then he infers from them *en masse*, that the water contains *carbonate of soda* as well as carbonate of magnesia: " All these phenomena, he says, as well as the discharge of air by the addition of oxalic acid, are easily explainable upon the presumption of the presence of the *carbonate of soda* and the carbonate of magnesia; and they seem inexplicable upon any other principle." First, he says virtually, that the experiments he had published would not " *warrant us in concluding*" that the water contains soda; and afterwards he says those very experiments do warrant us in concluding that very thing. First he says, all the phenomena indicate one thing, and then again they indicate another thing. At one time they prove the presence of magnesia and not soda; at another time they prove the existence of soda as well as magnesia. Originally they were *inexplicable*, upon the presumption of the presence of any other than the first mentioned material; but now they are *easily explainable* upon that very presumption, nay, they are *inexplicable* without it. But what is the consequence of this variation in Dr. S.'s inferences? It is simply this, that in 1807, with these experiments before him, he admits the Ballstown water *may* contain *muriate of magnesia*, according to the French analysis: But now it is impossible they should contain that substance, because he says it is incompatible with the " *carbonate of soda;*" the existence of which he has contrived to infer from the very same experiments he then made, by the help of a confirmatory one, which he has since inserted. These singular fluctuations and variations of opinions and conjectures, however, show the extraordinary resources of our author's mind. As he had proved that the *Saratoga* waters contained no muriate of magnesia, because they contained soda; so the Ballstown water, if it contained this last substance, could not contain the first; and it would be very desirable for him to be able to use the same reasoning in the one case as in the other. Dr. V. had not mentioned *muriate of magnesia* in the Ballstown water; and Dr. S. seems to be convinced, a priori, that in fact it cannot be present; and being once convinced, he never wants, a posteriori, appropriate experiments, and inferences, and reasoning to confirm and establish his convictions.

Now, with all the foregoing errors and inaccuracies I have mentioned; with all the imperfections, variations, and contradictions on the face of his performances; with all the doubts, difficulties, and wanderings of his mind; with all his subterfuge, sophistry, and ignorance; this man has the vanity and the assurance to appear in the pompous capacity of a judge and an umpire, to decide the difference, in the analysis of the Ballstown water, between a French chemist and an American one; and this too without having himself made any thing like an analysis of this water: for it does not appear that he has made any other experiments than the partial ones he has published; with a view not to ascertain the presence of any substance in the water, but to disprove the existence of *muriate of lime* and *muriate of magnesia.* He says, indeed, in his second edition, of the ten experiments he enumerates in order, that they were made "to ascertain still further what other matters were contained in this water;" but when he first published them in the Medical Repository, in 1807, he says no such thing. On the contrary, the seventh and eighth, as they are numbered in his second edition, and also the one with oxalic acid, he states with a view to settle the difference between the two chemists; and the remainder, except the tenth, he mentions to prove that in the course of his experiments with the aforesaid view, he had incidentally inferred the presence of magnesia. The tenth he does not mention in his first essay, and that perhaps is the only one made with a view to discover any particular substance, or rather to confirm his presentiment, that the water contained *carbonate of soda;* for he had previously mentioned it as being one of the materials in the Saratoga water; and if he could prove it to be also in the Ballstown water, it would be a convenient discovery, and would furnish him with the means of making a well-timed inference, that hence it was *impossible* for muriate of magnesia to exist in it. He accordingly makes the discovery, and the inference follows of course.

Dr. S. then gravely undertakes "not merely to settle the difference between the two experimenters, but *the more important purpose* of ascertaining what in truth are the real contents of the water;" and, for the accomplishment of these purposes he says, *justice to Dr. Vandervoort, and duty to the community,* require that he, Dr. S. should examine the water *more particularly.* But how has he examined the water more particularly? With respect to the *quantity* of carbonic acid gas, he has examined the water in no other way than by *reasoning* concerning it. And with respect to the *quantity* of all the other ingredients, he has not examined it at all. He has, indeed, enumerated the materials he supposes it to contain, but has not given the proportions of any, except his conjectural proportion of carbonic acid:—and he has not even done Dr. Vandervoort the justice to state the *quantity* of the several materials which that chemist procured, and mentioned in his analysis. Yet this is the result of Dr. S.'s more *important purpose* of ascertaining what in truth are the *real contents* of the waters. Does our author really think it of more consequence to the *public* to know that the water is *deficient* in very inconsiderable portions of muriates of lime and magnesia, than to know the *quantity* of the more abundant and more active materials of muriate of soda and carbonate of iron? Does he really believe it a circumstance of great importance, whether a given quantity of the water contains a few grains of muriate of magnesia or not? and a thing of no importance, whether it contains a grain or an ounce of carbonate of iron? Surely he cannot be so entirely devoid of judgment;—and, instead of *doing his* "*duty to the community*," he must be conscious that he has practised an imposition upon them, by pretending to examine the waters *particularly*, and to ascertain what their *real contents* were, and, at the same time, presenting the world with an account so totally deficient in the most important and essential point.

But what is the difference between the analysis of Dr. Vandervoort and the French chemist? According to Dr. S. it consists in three particulars. 1st. With respect to the quantity of carbonic acid. 2d. With respect to the presence of muriate of lime. And, 3d. With respect to muriate of magnesia. And it cannot be discovered from his statement, that there is any other disagreement. The reader, therefore, takes it for granted this is all the difference between the two analyses; but let him not trust to any presumption arising from Dr. S.'s statement, for if he does he will be often deceived. There are other points of difference more important than those which our author has given us, and which he seems to have studiously concealed in his second edition, by omitting altogether the French analysis in relation to the *muriate of soda*, the *carbonate of lime*, and the *carbonate of iron;* and also, omitting the *quantities* of each of these materials procured by Dr. Vandervoort. His apparent anxiety to conceal these essential differences in the two analyses, with respect to the proportions of the foregoing ingredients, may, perhaps, account for his not doing justice to Dr. V.'s statement on this occasion. But while one author wrote professedly with a view to settle the *difference* between the two experimenters, why has he suppressed, and withheld from the public, some of the most material points of difference? Was he not prepared to do his *duty to the community* with respect to these differences as he had promised, and therefore thought it most consistent to put them out of view altogether? While he professed to examine these waters *more particularly* than his predecessors had done, and found it more especially incumbent on himself to ascertain what, in truth, were their real contents, that those who *used* them, and the *Physicians who prescribed* them, might be the better regulated in their judgment, both as to the quality and *quantity* of the water to be used, how happened it that even *he* did not ascertain the *quantity* of these *contents?* Had he not time to make the experiments for that purpose? or did he, once in his life time, conceive the modest and becoming idea that he was incompetent to do it? Some one or other of these things must have prevented him from attending to the important particulars I have mentioned, and charity inclines me to ascribe it to the last, because it is the most honourable to the author, and the only excuse that can be satisfactory to the *community.*

As Dr. Seaman has not thought it proper to give a comparative view of the two analyses in question, I shall take the liberty to do it for him, accompanied also with an account of the Saratoga water; and I have only to regret that I cannot add to it his analysis of the Ballstown water, by which something might possibly have been done to settle the difference between the two first, or at least to make the errors in them appear more conspicuous, if he had only been so obliging as to have given us the quantity of any one, or of all the materials. In the annexed table the proportions are calculated, from the quantities given in the several analyses, as stated in the preceding part of this inquiry.

Statement of the quantity of ingredients in 10lb. by weight, estimated at five quarts by measure, equal to 288.75 cubic inches of the Ballstown and Saratoga waters, which measure of carbonic acid gas, at the temperature of 60° barometer 29.8, weighs 134.26875 grains.

In 10 pounds, or 10 pints, of water, equal to 288.75 cubic inches,	Ballstown Water.		Saratoga Water.
	French analysis.	Dr. Vandervoort's.	Dr. Seaman's.
Carbonic acid gas - -	866.25 cub. in.	270.83	200 cubic in.
(equal to) - - -	402.8 grains	125.93	93 grains.
Muriate of soda - - -	198.4	180.	173
Carbonate of lime - -	140.8	166.25	190
Carbonate of iron - -	25.6	19.37	8.5
Muriate of lime - -	32		
Muriate of magnesia -	80		
Carbonate of soda - -			26
Carbonate of magnesia			A little.
Sulphureous impregnation			Ditto.

Having taken this *view of the whole ground*, I am now prepared to consider the points of difference which Dr. S. has noted. With respect to the others I shall rest satisfied with having brought them back from that oblivion in which he had buried them. The public will make their own comments. A remarkable disparity will be observed in the proportions of the *iron;* but upon this point Dr. S. is *silent;* and I shall torture him no longer. His proportion of carbonate of lime also is much larger, and that of his marine salt much less than either of the others. Possibly this may be owing to the remarkable want of solvent power in his chemical agents. As his watery spirits would not dissolve one of the most soluble of all soluble things, the mineral alkali, it is not to be wondered at that his insoluble residuum should be very large. Possibly also he may have weighed his lime after he had converted it into *gypsum*, and thus included the weight of his vitriolic acid. This would have been exactly conformable to his mode of proceeding with respect to the iron, in which he included equal its weight of prussic acid. In his first edition, he says he used *distilled* water as a menstruum in some of his operations, but in his second edition he has changed it to *water* simply. Now, as it is a matter of some importance, in relation to the accuracy of our author's experiments, he would have done well to have told which of his two statements is the *true* one, and whether he absolutely used *cold water* only, or *distilled water*, and, if the latter, whether it was distilled from a *retort*, from the *earth*, or from the *clouds*. With respect to that insulated article, the *carbonate of soda*, it is very questionable whether either the

Saratoga or Ballstown waters contain it. Our author's inexperienced optics cannot be trusted to distinguish the shades of colour which that or the alkaline earths may produce on *turmerick.* This experiment he has inserted in his second edition, though it is not in his first. The existence of the *sulphureous impregnation* is also very doubtful. The "*dirty bluish colour*" which the precipitate by nitrate of silver assumed *soon* after the experiment was made, and which Dr. S. supposed to be produced by a sulphureous impregnation, was undoubtedly the effect of light, and not of sulphur. Light produces precisely the effect which followed his experiment, but the precipitate of sulphureous impregnation is "*reddish, or brown, or black:*"* so that Dr. S. appears to have drawn a "*highly erroneous*" conclusion from this experiment.

But I proceed, without further delay, to the first material point in dispute, concerning the quantity of the *carbonic acid gas.* This Dr. S. states in the Saratoga water at 200 cubic inches in 10lb. that is, 88.75 cubic inches, or more than a quart and a pint less than its bulk; although both his previous *conclusion* and his *experiment* afterwards make it about *equal* to its bulk. But 100 cubic inches of water, at the temperature of 60°, absorbs 108 cubic inches of fixed air or carbonic acid gas; 10lb. of water, therefore, or 288.75 cubic inches, will absorb 311.85 cubic inches of this air: and this is exactly the quantity chemists mean when they say water absorbs *about equal its bulk* of fixed air. Dr. S. in his account, refers to Henry, on whose authority we have this fact: he concludes the Saratoga water is fully *saturated*, and by his *experiment* he procured *upwards of its bulk* of air; and yet, in his statements he does not come up to the quantity which water absorbs by 111.85 cubic inches; a deficiency of nearly half a gallon in one gallon and a quart. Does Dr. S. believe the quantity he procured over and above 200 inches to be *oxigen* and *azote?* Then he believes an impossibility. And if he is ignorant of the facts on this subject, that ignorance can be his only apology for his erroneous statement; if not, he must submit to the alternative of knowingly and wilfully imposing a mistatement on the public. I leave him to take his choice of difficulties. One hundred cubic inches of water, at the temperature of 60°, barometer 29.8 absorbs only 3.7 cubic inches of oxygen gas, and 1.53 of azotic gas; so that 10lb. or 288.75 cubic inches of Saratoga water, if it contains either of these airs, which it is not proved to do, cannot contain more than about 10.6 cubic inches of oxigen, and 4.4 of azotic gas, making together about 15 cubic inches. But how great the disparity between 15 and one hundred and eleven! Yet Dr. S. says he obtained *upwards of its bulk* of air from this water, the greater part of which he took to be carbonic acid gas; the rest *perhaps* atmospheric, mephitis, (azotic gas) and partly vital air, or oxigen gas. How wonderfully nice and particular our great experimenter must have been, and how accurate in his conjectures and his conclusions! His *statement* absolutely comes within *one hundred and eleven inches* of his *experiment;* between that and his *conclusion* we may safely interpose *one hundred and eleven inches* more; and, if I mistake not, his *conclusion* is even then at least *one hundred and eleven inches* from the TRUTH!

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* Henry's Chemistry, p. 317, 318.

When Dr. S. first wrote his dissertation in 1793, he does not appear to have known that water could be made to absorb much more than about equal its bulk of carbonic acid gas: and if so, then he very naturally thought it impossible for any mineral water to contain more. It was necessary, therefore, that he should conform the result of his experiment, in some way or other, to his preconceived notions. And without allowing for what may have been absorbed by the water over which he collected it, he presumes the additional quantity he notwithstanding procured over and above the bulk of the water, to be *vital air* or *mephitis*, or any thing else to keep within proper bounds. But now it is no longer necessary, on account of *consistency*, for which Dr. S. is so conspicuous, to adhere to his ancient opinions. By the modern discoveries in chemistry he is indeed driven from this ground of ignorance, by which he was originally fortified; and he is obliged to seek some other refuge: yet, with the light of science dazzling his eyes, he appears to grope in the dark, and holds fast to his errors. "*Neither Berghman nor Henry,*" he says, *could cause water, at the temperature of the Ballstown waters, to absorb more than about that proportion.*" that is, equal its bulk: and, therefore, he concludes these waters cannot contain more. But let us see whether Henry could not cause water to absorb more than that quantity. "From the experiments of Mr. William Henry (says Thomson*) upon gasses subjected to different degrees of pressure, from that of one to that of two or three atmospheres, and thus reduced to double or triple their usual density, that philosopher has deduced the following very important law: water of the same temperature always takes up the same bulk of each gas, whatever be its density. Thus, if we suppose that water at 60° absorbs just its bulk of carbonic acid gas in its ordinary state of density, it will still continue to absorb its own bulk, though that gas be condensed into half its usual space, or one third its usual space, and so on. Hence it follows, that by increasing the pressure sufficiently, we may cause water to absorb any *quantity* of gas we please. To cause water to absorb what is equivalent to twice its bulk of carbonic acid, we must make it to absorb the gas under an additional pressure of 30 inches of mercury: to make it absorb what is equal to thrice its bulk, we must subject it to a pressure of 30 inches more, and so on." Mr. Thomson has further shown, by a train of very ingenious reasoning from the known properties of this air, and the laws regulating its absorption, that "*water will absorb sixty-four times its bulk of carbonic acid.*"

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* Vol. iii. p. 496.

Now, in relation to the Ballstown water, Dr. S. is so obstinately wedded to his antiquated notions, that he cannot conceive, notwithstanding the *incalculable pressure* which he admits it may be subjected to in its confinement under the earth, how it can be supersaturated with carbonic acid at the moment it issues from the earth into an "*open spring.*" He cannot believe that this *open spring*, continually furnished with a fresh supply of water, supersaturated with this air, can contain more than equal its bulk. This objection is not only absurd, but ridiculous: as well might he attempt to convince one that the temperature of these *open springs* is equal to the surrounding atmosphere in a hot summer's day; that because heat has a constant and rapid tendency to an equal distribution, and is continually passing from the atmosphere to the *open spring*, therefore it is impossible the thermometer should indicate it to be 60°, while the surrounding air is at 90°. If, indeed, the constant supply of water was cut off, and the springs should become *stagnant* as well as *open*, then their temperature would rise, and their fixed air entirely escape; but so long as there is a living fountain, a "*cold boiling*" stream, and men retain their feeling and their taste, a thermometer will not be wanted to convince them that the water is colder than the atmosphere, nor a "*syphon and a Florence flask*," that it is supersaturated with carbonic acid. And whoever should seriously attempt to impose a contrary belief, by sophistry or assertion, would be liable to the suspicion of having not only his judgment impaired, but his senses vitiated.

To all that Dr. S. has said on this subject it will be sufficient to oppose the common taste of mankind, in order to overturn completely all his conjectures. Whoever has tasted water impregnated with equal its bulk of carbonic acid gas, by means of simple *agitation*, (the method by which Henry caused it to absord more than its bulk); or whoever has tasted water supersaturated with this air by means of the pressure of Nooth's apparatus, and also the Ballstown water from the springs, will not hesitate a moment to pronounce which contains the most fixed air. The latter is by far the most pungent to the taste. And when it has lost its superabundant fixed air, by being kept for some time out of the springs, exposed to the atmosphere, it cannot be again restored to its original taste, either by being agitated with carbonic acid gas, or even by its confinement in Nooth's apparatus. With all the additional quantity of air that can be forced into it by the pressure of this machine, its taste is still flat and insipid, when compared to the sparkling water which issues from the spring. If, moreover, any judgment can be formed, by those who are conversant with the action of this air upon water, of the quantity combined with it, by the rapidity and force with which it escapes while the water is gushing from the earth, and by the abundance of air continually extricated in this manner; then no one can look at the *Congress spring* of Saratoga for a moment, without being unalterably convinced that the water contains a very great quantity of fixed air, over and above the quantity it can absorb under the ordinary pressure of the atmosphere, and consequently that it must have been subjected to very great pressure within the earth. The air does not simply *bubble through* the *water*, as Dr. S. says of the Rock spring; but it is extricated *from the water itself*, and from its *whole surface*, in fine parcels, with great rapidity, and more abundantly than air escapes from boiling water. The water of this spring indeed is perceptibly, and but just perceptibly more pungent than the other waters. But, if the Ballstown water does not contain more carbonic acid than water can be made to absorb under the common atmospheric pressure, how happens it that chemists have not until lately furnished us with water so lively and agreeable as these natural waters? How happens it that we are obliged to resort to mechanical powers, operating with prodigious force, in order to produce an artificial water impregnated with fixed air, that approximates to the pungency of the Ballstown water? Will Dr. S. with all his sophistry, attempt to account for this fact? or will he not contend for the *dust of the balance?* As his measure regards not one hundred and eleven cubic inches, I know not what his taste may indicate on this occasion.

There is but one additional observation of our author that requires notice. He says, that "in Saunders's account of the mineral waters of Europe, there is not a single one stated to contain as much as equal its bulk of carbonic acid gas in its composition: that of the Pyrmont spring, which appears to be the most highly impregnated, contains only 26 cubic inches of the gas to 28.875 of the water." This quantity of gas, it is to be observed, although not quite equal to the bulk of the water, is 60 cubic inches more in 10lb. of water than Dr. S. states the Saratoga water to contain—and yet he says the Saratoga water is fully saturated, and contains about equal its bulk. To have made this account of Saunders then conformably to his own, he should have said the Pyrmont water contains 60 cubic inches more than its bulk: for surely if 200 cubic inches will saturate 10lb. of Saratoga water with nearly equal its bulk, then 260 cubic inches will supersaturate the same quantity of Pyrmont water with nearly 60 cubic inches more than its bulk. But we have heard enough of Dr. Seaman's consistency: and whatever Saunders may have mentioned concerning the Pyrmont water, I find, that according to Thomson, both Berghman and Westrumb state it to contain more than equal its bulk of carbonic acid gas. Berghman's analysis is thus stated, 8950 parts by weight of water to 19.6 of carbonic acid gas. Now, 8950 grains of water are about equal to 36.91 cubic inches, and 19.6 grains of carbonic acid gas are equal to 42.14 cubic inches: so that in 288.75 cubic inches of Pyrmont water, there would be 326.95 cubic inches of gas, or 38.20 cubic inches more than its bulk; and 126.95 cubic inches more than Dr. S. supposes the Saratoga water to contain. "Westrumb says 100 cubic inches of Pyrmont water contain 187 cubic inches of carbonic acid gas, or almost double its own bulk." Klaproth also found 107.50 cubic inches of this gas in 104.44 cubic inches of the water of Carlsbad; or 296.25 cubic inches of air in 288.75 of water. This analysis is stated by Thomson at 25320 parts of water to 50 of gas, and conformable to this is the above calculation. Now, all these facts are mentioned by Thomson, without being once questioned or doubted; nor is there to be found, in any modern book of chemistry, that I know, so ridiculous an idea, as that a mineral water cannot contain more than equal its bulk of carbonic acid gas. The Pyrmont water I have never tasted at the spring; so that I cannot tell whether it is equal in life and pungency to the Saratoga. But I have never heard it represented to be more so, and, indeed, I think it impossible it should be; yet Berghman and Westrumb both affirm it to contain more fixed air than water can absorb under ordinary atmospheric pressure; and Dr. S. by his own statement from Saunders, makes it contain 60 cubic inches more than the Saratoga water. How far from the truth then must Dr. Seaman's account be; and how shallow and contracted his notions. Yet, strange as it may appear, and arrogant in the extreme, this man pretends to settle the difference between a French chemist and Dr. Vandervoort, concerning the Ballstown water. And he does it in the twinkling of an eye. Like Cæsar of old, he came, he saw, and he conquered. Dr. V. concludes, from *experiment*, that the water contains equal its bulk of fixed air. The French chemist concludes, from *experiment*, it is to be presumed, that it contains three times its bulk; and Dr. S. concludes, *without any experiment*, that the first is right, and the second is wrong. The Reviewer, on the contrary, does not say that either is right. The probability indeed is, that they are all wrong. But however this may be, one thing is certain, and that is, that the statement of Dr. S. to use his own words, is *highly erroneous*. And another thing is also very certain, to wit, that he has not succeeded in his herculean project of settling a dispute, to do which he was totally incompetent, and with which he had no business. The Reviewer, therefore, stands amply confirmed in his mild and dignified sentence, that Dr. S. had failed in his undertaking; for that reasoning alone could never decide a matter of fact.

Another point of difference mentioned by Dr. S. is concerning muriate of lime. The question is, whether the Ballstown water does, or does not contain this substance. Now, the following method is prescribed by Thomson to detect the presence of muriate of lime in mineral water. "Free the water of sulphate of lime and other sulphates, by *evaporating it to a few ounces*, mixing it with spirit of wine, and adding, last of all, nitrate of barytes as long as any precipitate appears; *filter off the water*, evaporate to dryness, treat the dry mass with alcohol, evaporate the alcohol to dryness, and dissolve the residuum in water. If this solution *gives a precipitate* with acetate of silver and *oxalic acid*, it MAY contain muriate of lime. It must contain it in that case, if, after being treated with *carbonate of lime*, it gives no precipitate with ammonia. If it does separate the lime by means of oxalic acid, filter and distil with a gentle heat. If the liquid in the receiver gives a precipitate with nitrate of silver, muriate of lime existed in the water." But Dr. S. appears to be more acute, at least in his own estimation, than Thomson or any one else. He discards all rules and methods except his own, and when it requires a long and laborious process to untie a gordian knot, he takes the short way, and cuts it at once. He boils down the water, pours in the oxalic acid, and instantly proves, in as many *minutes* as any other chemist would require *days*, that there is no muriate of lime in the water, because no precipitate is produced. If this short and easy method of Dr. S. is an infallible one, as he represents it, how happens it that Kirwan, and Thomson, and other eminent chemists have not used it? The reasons are evidently these: 1st. Lime may exist in water, and not be precipitated by the oxalic acid. The presence of the mineral acids in excess will prevent it; some of them decompose the oxalic acid, and others dissolve the oxalate of lime. 2d. Oxalic acid precipitates magnesia as well as lime. And, 3d. Water, after being boiled, may still contain sulphate of lime, muriate of lime, nitrate of lime, and even a minute portion of carbonate of lime; and also sulphate, muriate, nitrate, and carbonate of magnesia. In Dr. Seaman's experiment, therefore, on the Ballstown water, if no precipitate appeared, it would be no infallible indication of the *absence* of lime; and if a precipitate appeared, it would not infallibly indicate the *presence* of lime, because that precipitate might be magnesia; and, much less would it indicate exclusively the existence of muriate of lime, for it might be produced by either sulphate, nitrate, or carbonate of lime. But Dr. S. admits the presence of both lime and magnesia in the water before boiling; and as neither of them can be wholly extricated by boiling alone, which will be presently demonstrated, his experiment was not only insufficient, but it was inconsistent, and evidently erroneous, from the very face of it; for, if he supposed the carbonate of lime to be altogether precipitated by boiling, he evidently did not suppose the whole of the carbonate of magnesia to be extricated by that operation, otherwise he would not have made his experiments to discover its presence in the water after being boiled. The oxalic acid, therefore, if it had found no lime to precipitate, would have precipitated the magnesia, and a cloudiness would have been produced; still he says, in his experiment, no cloudiness or precipitation appeared. Now, the Reviewer, from too much delicacy and forbearance, as has since appeared, and from personal respect to the author, did not wish to expose him to ridicule by entering into particulars; but stated only a general fact, that might be presumed to have escaped Dr. Seaman's observation without much disparagement to his science; which fact, as it indicated the want of accuracy in his experiment, was supposed, if he had sense enough to comprehend it by a little investigation, might lead him to take the proper method of detecting the muriate of lime; and, in the mean time, the public would be sufficiently apprized that he had not yet settled the difference between the two chemists. The fact mentioned was, that *boiling alone could not have extricated the lime from the water*, or, in other words, that after boiling, there would still be left so much lime in the water as would be sensible to the extremely delicate test of oxalic acid; and, the inference was, that the author's experiment, if there was no error in it, would prove that there was no *lime in any shape* present in the water. Instead of investigating this fact, and thus meeting the Reviewer's argument, Dr. S. shrinks from the force of it, and resorts to the shameful subterfuge of misrepresentation. He represents the Reviewer as saying a thing that would make an ideot smile, and then Dr. S. smiles at it accordingly. He sets up a phantom of his own imagination, and points his finger to it, as to a real existence. And he prefers fighting his own shadow with ridicule, rather than combating the Reviewer with logic. He makes the "*public Censor*," "assert that *lime held in solution by the agency of the carbonic acid* would not be precipitated by boiling the water." But the Censor has asserted no such thing. Every salt-boiler, indeed, knows that much of the lime so dissolved is precipitated by boiling; and he may suppose too, that *all the lime* so dissolved is extricated. This, however, is the extent of his knowledge, and so also it appears to be of Dr. Seaman's. But has not *water*, as well as *carbonic acid*, some *agency* in dissolving lime? Is the water altogether *inert*, and the carbonic acid only *active* in this operation? Can water, without the agency of carbonic acid, dissolve lime? And can it not dissolve a particle of lime when that lime is saturated with this very agent of solution, the carbonic acid? If there is a particular point of saturation, at which carbonate of lime is supposed to be insoluble, will it not be soluble immediately above and below that point? Will boiling always expel just so much carbonic acid from supersaturated carbonate of lime, as to bring it to that imaginary point? And how much or how little soever it may be boiled, will it never vary above or below that point? Is not crystallized carbonate of lime at this perfect point of saturation? and how can it be crystallized unless it was previously dissolved in water? Are not silex and gypsum said to be insoluble? and yet are they not found, in common with carbonate of lime, dissolved in many mineral waters? and are they not also found in a crystallized state? What do writers commonly mean when, in general terms, they speak of soluble and insoluble things? These inquiries, perhaps, never entered into Dr. S.'s mind. He appears, indeed, to have read much faster than his understanding could follow; and when he finds himself bewildered, and lost in a wrong course, although he looks at the compass, he calls north south, and south north. No wonder then that he should have given a preposterous turn to a plain statement of a simple fact. He certainly does not conceive that the expression, " boiling alone could not have extricated *it*," that is to say, the *whole of it*, meant that *no part of it* could be extricated: and it is evident that he did not so understand the Reviewer; for he makes a quotation to show that ærated lime is *entirely* precipitated by boiling, in opposition to the idea that it is *not entirely* precipitated, but that a minute portion is still held in solution after boiling. If, therefore, Dr. S. when he says the Reviewer is " *so* ignorant as to think, and so bold in his ignorance as to assert, that lime held in solution by the agency of the carbonic acid would not be precipitated by boiling the water," means to impute to him the idea, that boiling would not extricate any part, or even the greatest part of the lime, he contradicts himself, and denies a thing which he has tacitly admitted he does believe. If, moreover, the author thinks, as he appears to think, that all the carbonic acid, by the *agency* of which the lime is held in solution, will be expelled by boiling alone, then to say that boiling would not precipitate the lime, would be a solecism; for where there is no *agent*, there can be no solution. This solecism, however, Dr. S. invents, and ascribes it to the Reviewer; but the Reviewer has not said any thing about the agency of carbonic acid in dissolving the *lime*, of which he speaks; on the contrary, he believes *water* has an *agency* in dissolving *lime* and carbonate of lime, as well as carbonic acid has; neither has he said any thing to authorize Dr. Seaman to take it for granted, that boiling will expel all the carbonic acid by the *agency* of which lime is dissolved; and much less has he said that no lime will be precipitated from a solution of supersaturated carbonate of lime. So much for our author's *candour* and *truth.* As to his *gentility* and " *urbanity*," I need say nothing here; he has shown that he has an exclusive title to these: and his *knowledge* he has displayed so conspicuously in his whole pamphlet, as to give him the undoubted privilege of calling every one else *ignorant and unlettered.* His declarations too, on this subject, the world are bound to believe as firmly as they believe his *experiments*, his *statements*, his *conclusions*, his *facts*, and his *representations.*

Dr. Seaman, it appears, has read in books that carbonate of lime is *insoluble* in water, and so also he has read that gypsum, and fluate of lime, and silex, and many other materials, are insoluble, without considering that there may be some authors like himself, who speak in the *large way*, and do not contend for the *dust of the balance.* These authors, if they understand the facts, when they speak in general terms, according to common language, of the insolubility of these things, can only mean that they are apparently, or comparatively insoluble. Water is so universal and powerful a solvent, that it takes up so much of most materials as to be immediately obvious to our senses. We can either see or taste a change in the water, or we can readily ascertain the quantity of the substance dissolved. But of the more insoluble things, the quantity held in solution is neither apparent to the senses, nor can it be easily estimated. Hence writers of scrupulous accuracy use the terms, *difficult of solution*, *not sensibly soluble*, *solubility not estimated;* and, with respect to the carbonate of lime, Mr. Murray, of Edinburgh, says, it is *nearly insoluble*, thereby admitting its solubility. Indeed, it is not to be presumed, that any chemist of comprehensive views ever supposed carbonate of lime to be *perfectly insoluble:* for the fact that it is found in large masses, in a crystallized state, by the intervention of water, in which it must have been dissolved, sufficiently demonstrates its solubility; yet it is perhaps impracticable, by artificial means, to dissolve any sensible portion of crystallized carbonate of lime; and in this view it may be said, without any manifest impropriety, to be insoluble; while, at the same time, it is in fact known to be soluble.

But Dr. Seaman, in his wisdom, says, the *lime will be extricated from the water by boiling alone*, because it expels the carbonic acid by which it was held in solution. He will not deny that lime, without the agency of carbonic acid, is soluble; nor will he deny that lime, with a small quantity of carbonic acid, is soluble. And he has admitted, that super-carbonate of lime is soluble: there must then be an intermediate point, at which carbonate of lime is insoluble. Supposing then the existence of such a particular point of saturation or neutralization, which, however, cannot be admitted from any facts we are acquainted with, it has never been ascertained that the heat of boiling water, or 212°, is precisely the temperature which will bring it to that particular point: and if that degree of heat expels either more or less than the exact quantity of carbonic acid required to make it insoluble, it will in both cases become soluble to a certain extent. Now, it is very remarkable, that nature has provided no means to boil her water so as to produce insoluble carbonate of lime. The temperature of the earth is at least 150° below the boiling point, and yet our learned author seems to think there is insoluble carbonate of lime produced by nature, as well as by his boiling heat. On his own presumption then, the point of insolubility of carbonate of lime is a critical point not ascertained; and on a fair calculation of chances, it would be ten thousand to one that he should ever hit it; surely the insolubility of lime is Dr. Seaman's *point no point.*

As lime is soluble in its pure state, by the agency of water alone, without the intervention of carbonic acid; as, moreover, subcarbonate of lime is soluble, and also supercarbonate of lime; and as carbonate of lime, in its intermediate and most perfect state of neutralization, has likewise demonstrated it to be soluble, we cannot escape the irresistible conclusion, that lime is more or less soluble in every state of combination with carbonic acid.

But with respect to Dr. Seaman's experiment with oxalic acid, on the Ballstown water, there is no further occasion to reason. I can now positively say that it was *fallacious*, and the result was *erroneous*, according to the prediction of the Reviewer. In the Ballstown water, boiled down to one fourth and one fifth its original quantity, and filtered, the *oxalic acid*, and also the oxalate of ammonia, *will produce a cloudiness and precipitation:* and even the sulphuric acid renders it visibly turbid. Now *this* is exactly what every chemist, except Dr. S. would naturally expect: and if he wants his "*single authority*" on this subject, I can refer him to the Professor of Chemistry in the University of this State. That gentleman carefully prepared his own tests according to the most approved methods; he used concentrated solutions of oxalic acid, and oxalate of ammonia, and he has repeated the experiment over and over again with the same result. Dr. Seaman is now left to find out the cause of error in his own experiment if he can. If his own hand is too rude to manage a delicate operation, he can always employ a competent one to do it for him; and if his optics are too gross to perceive the result, he may purchase a pair of spectacles to aid his vision. For any other chemist, however, this will be unnecessary. If the experiment is accurately made, no one can fail in producing the effect I have mentioned. But Dr. S. will be too obstinate to be convinced against his better knowledge. My only object is to present the truth to the *public*, that they may judge for themselves.

There is one other point of difference between the two chemists, which Dr. S. vainly attempts to settle; and this requires some further notice. The French chemist says the water holds in solution muriate of *magnesia;* and Dr. Vandervoort makes no mention of this composition. Now, nothing seems to have puzzled Dr. S. so much as this same simple substance, magnesia and its compositions. I have already mentioned his contradictions and variations of opinion on this article. He appears to have been lost in the magnitude and intricacy of the subject. It evidently was beyond the reach of his mental powers, and they constantly relapsed into a state of confusion. He thought he discovered carbonate of soda in the water, and he had read that carbonate of soda, and muriate of magnesia, were incompatible salts. But here too, as well as on another occasion, he appears to have read without understanding his author. It is true, there are incompatible salts enumerated by writers on chemistry, that is, salts that cannot exist together in the same solution, because they decompose one another to a certain extent. Thus *carbonate of soda* and *muriate of lime* are incompatible; and yet Berghman found both these salts together in the waters about Upsal. Thus *carbonate of magnesia* and *muriate of lime* are incompatible salts; and yet Garnet found them both in the Harrowgate water, and Schmeisser in the Kilburn water. Thus *sulphate of soda* and *muriate of magnesia* are incompatible salts; and yet Fothergill found them both in the Cheltenham springs, and Carrack in those of Bristol. Thus *carbonate of magnesia* and *sulphate of lime* are incompatible, and, according to Dr. Seaman's notion, cannot exist together in the same solution; and yet Berghman found them both in the Pyrmont waters, and Fourcroy in those of Enghein. Thus I might proceed through the whole list of salts; but with respect to incompatible compounds, this qualification must always be kept in view, as laid down by Kirwan and Thomson. They cannot exist together in water, *except in small quantities*. Now, this state of facts overturns the whole foundation of Dr. Seaman's reasoning, and he must be ignorant indeed, if he supposes incompatible salts, so called in general terms, to be altogether incompatible. But this is a counterpart of his accurate notions, with respect to the total insolubility of carbonate of lime. Admitting then, for argument's sake, that the Ballstown water contains carbonate of soda, it does not follow that it cannot contain also a small quantity of muriate of magnesia; although these salts, in large quantities, decompose each other.

Dr. Seaman says the Reviewers " prostrate a Berghman, a

Kirwan, a Murray, and a host of other drudging experimenters at their feet." He mistakes—the Reviewers have not attempted to do this, unless he ranks *himself* amongst the host of drudging experimenters, and then they will have prostrated only one who is not entitled to that rank. But the constellation of French chemists have prostrated Berghman's system of chemical affinity; and if Dr. Seaman will read *Murray's* system, he will find this to be the case. Berghman's whole system is founded on the absolute uniformity and efficacy of elective attraction. Thus, for example, on his principle, when an acid and an earth are combined, the addition of an alkali, having a stronger attraction for the acid than the earth, will unite with the acid, to the complete exclusion of the earth, and the earth cannot afterwards separate any part of the alkali from the acid. According to Berghman, affinity is electrive, and decomposition measures the intensity of the affinity between bodies. But, according to the subsequent discoveries of Berthollet, affinity is not electrive; nor is it a constant force, since it varies with the *quantity* of the acting bodies, quantity being capable of supplying the place of affinity; decomposition, therefore, cannot be considered as the measure of the intensity of affinity. " A substance which has a stronger affinity is not capable of separating completely those which have a weaker affinity; or, if this happens, some other cause intervenes. Instead of separating the weaker body, it divides with it the base to which that body was united; each combining with a part proportional to the *strength* of the affinity and the *quantity* present. This new opinion is exactly the opposite of the old. According to the former hypothesis, when sulphuric acid is dropped into nitre, it separates the whole of the nitric acid, and takes its place. According to Berthollet, each of the acids combines with a portion of the base of the nitre, and the portion which unites to each is proportional to the strength of affinity and the quantity of each acid employed." This doctrine of affinity is very satisfactorily established, and almost universally admitted; there is scarcely an anomalous example to be found opposed to the general principle of the partition of one body between two or more others, according to their respective *masses* and *affinities*. But Dr. Seaman reasons altogether on the old exploded and refuted hypothesis of Berghman. He reasons from false premises, and, therefore, all his reasoning on this subject is false; yet, with this false reasoning, he very ludicrously attempts to overturn facts and experiments. How very different was the conduct of Berghman, Kirwan, Cavendish, and other eminent chemists, the very authors and supporters of this same hypothesis. They discovered, in various mineral waters, the co-existence of small quantities of various salts, which, in common circumstances, decompose each other, and which are universally considered as incompatible. These results too, militated against their favourite theory; yet they preferred truth to conjectures, and never exposed themselves to the derision of the world, like our more wise and learned author, by saying that it was *impossible* that salts should exist together, because of their peculiar chemical attractions. But, how does the established doctrine of affinity explain the association of incompatible salts in water? Take, for example, the Harrowgate water; this contains 13 grains muriate of lime, and 5 grains sulphate of magnesia, to one gallon. In this case, on account of the small quantity of salts, compared to that of the water, the affinity of this last, aided by its mass, is, no doubt, capable of overcoming the excess of the divellent over the quiescent attractions of the salts it holds in solution; and this is proved by the circumstance that boiling produces the decomposition of these salts in the water. By evaporating much of the water, the sulphuric acid unites to the lime, and the muriatic to the magnesia, and two new salts are formed, to wit, *sulphate of lime* and *muriate of magnesia*, from the muriate of lime and sulphate of magnesia existing in it before the operation of boiling. This circumstance too, relative to the effect of boiling, never seems to have entered Dr. Seaman's imagination. He boils down his water without mercy, pours in something he calls oxalic acid, and, behold, there is no muriate of lime in the water! If he had read this fact, he, no doubt, would have believed that boiling had extricated all the muriate of lime, as well as all the carbonate of lime. What will not this wonderful operation do? Surely, his *boiling water* will precipitate all his lime in every shape, as completely as his *boiling wrath* precipitates all his science.

According to Dr. S. the Ballstown water contains two acids, the *muriatic* and the *carbonic*. It also contains *lime*, *soda*, *magnesia*, and *iron*. With each of these four last ingredients the carbonic acid is combined; but the muriatic acid is united only with the soda. Now, it appears strange, and indeed irreconcilable with the principles of chemical affinity as established by experiment, that the whole of the muriatic acid should be combined with one of these materials, to the exclusion of all the others; that every part of it should adhere to the soda, and none to the lime and magnesia. How does this comport with the irrefutable fact established by Berthollet, that an acid divides itself amongst several bases, according to the quantity and the affinity of the bases for the acid? And how does it correspond with other analyses of mineral waters made by eminent chemists? In sea water, Mr. Lavoisier found the muriatic acid united to the lime and magnesia as well as to the soda; and Berghman found the same. In the springs of Upsal, Berghman also found muriate of lime and muriate of soda, as well as carbonate of lime and carbonate of soda. In the waters of Leming-Priors, Lambe found both muriate of soda and muriate of magnesia. In the waters of Lee, Bresi found muriate of soda and muriate of lime together, with carbonate of lime. In the water of Teplitz, Jahn found both muriate of soda and muriate of lime, with carbonate of soda and carbonate of lime. In the water of Kilburn, Schmeisser found muriate of soda, muriate of lime, and muriate of magnesia, and also carbonate of lime and carbonate of magnesia. In the Harrowgate water, Garnet found muriate of soda, muriate of lime, and muriate of magnesia, and carbonate of lime and magnesia. But why multiply examples to establish a proposition almost self evident? There is then the strongest evidence, that the French chemist, when he pronounced the Ballstown water to contain muriate of magnesia and muriate of lime, as well as muriate of soda, was correct in his analysis; and thus fortified, no one will disbelieve him, until there is better evidence, and higher authority opposed to him than Dr. Seaman and his book.

I shall now take my leave of Dr. Seaman, with recommending to his attention the following fable, which he probably has read, but may have forgotten. A crow, conceiting himself endowed with the powers of an eagle, resolved to imitate the exploits of that noble bird. Accordingly, he one day made a pounce at a lamb, but being too weak to bear away his prey, and his feet being entangled in the wool, so that he could not extricate himself, he was caught by the shepherd, who carried him home to his children. They eagerly demanded what bird it was? A short time ago, replied the shepherd, he thought himself an EAGLE, but probably he is by this time convinced he is a mere CROW.

Now, gentle reader, I owe you thanks for your patience in following me through this tedious investigation. Although the "*dissertation*" I have examined may be worthless, so far as it relates to the analysis of the water; yet the subject itself is interesting and worthy of consideration. There is some hope that the present examination may induce some chemist of competent abilities to give us a complete and satisfactory analysis of these celebrated waters; and should this desirable consequence follow, I shall claim some share of merit for having turned the public attention to this important subject.

* * *

Albany, Jan. 2, 1810.

To Dr. EDWARD MILLER,* *one of the Editors of the Medical Repository of New-York.*

SIR,

In the last number of your periodical work there is inserted a communication, addressed personally to me and to another gentleman, signed by Valentine Seaman, who, not content with having encountered the Reviewer, courts also an altercation with the "*Professor of Chemistry.*" In this, however, he cannot be indulged until he learns to write in the style of a gentleman. I have only to observe to you, for the information of the public, that I am truly so ignorant as to think, with the writer of the Review concerning the *lime* held in solution by the Ballstown water, that *boiling alone could not have extricated it;* for after all that boiling can separate is precipitated, there will be still so much left in solution, as can be detected by the very sensible tests of oxalic acid, and oxalate of ammonia, and this is precisely the idea of the Reviewer. He cannot be mistaken.

The solubility of carbonate of lime, in its most perfect state of neutralization, without any superfluous carbonic acid, is demonstrated by the fact of its being found crystallized through the intervention of water, in which it must have been previously dissolved. This carbonate of lime, if it could be decompounded by the heat of boiling, would only, thereby, be made to approximate the nature of pure lime, which is more soluble; and it is not pretended that sulphate, muriate, or nitrate of lime will be precipitated; so that *lime in any shape*, when once dissolved in water, cannot be altogether extricated by boiling alone, unless, indeed, it may be said to be extricated when the whole of the water is evaporated. The experiments with the oxalic acid and the oxalate of ammonia, I have frequently repeated on the Ballstown water boiled to one fourth, one fifth, one eighth, and one tenth its original quantity, and filtered, and always with the same result; a visible cloudiness and precipitation are instantly produced in the water, and this cloudiness is not destroyed by the sulphuric acid; nay, this acid alone renders the water perceptibly turbid. I carefully prepared the oxalic acid, according to the most approved method, and I used saturated solutions of it, and of the oxalate of ammonia.

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* Dr. De Witt is mistaken in supposing that Dr. Mitchill was not privy to this breach of decorum. *Edit.*

As you have referred personally to me, on this subject, it may be some additional satisfaction to you to be informed that I have examined Dr. Seaman's Dissertation on the Saratoga and Ballstown waters, and have no hesitation in saying, that it is not a correct performance, indeed, it abounds with inaccuracies and errors from the beginning to the end; but the Reviewer understands the subject too well to require the sanction of my name for the purpose of exposing Dr. Seaman; and a full examination of his singular pamphlet and answer to the Reviewer, may be expected in the 3d number of the Medical and Philosophical Journal and Review. In the mean time, you will do *me* the justice to insert this letter in the Medical Repository, and I leave it to your sense of honour to make an apology to the public, in behalf of yourself and your co-editor, for having *commenced*, in a literary journal, a personal attack, which you must be conscious is improper and indecorous.*

I am, Sir, with due respect and consideration,

B. DE WITT.

Notice to Correspondents.

The whimsical speculations signed "V. Seaman," concerning who may be, or who may not be the writer of a review in a former number of this Journal, are inadmissible; for the public can have no interest in such idle animadversions. It is sufficient for V. S. to know, that this work is conducted by an association of gentlemen, in different parts of the United States, as the prospectus purports; that the reviewing department, particularly, is attended to by a considerable number of men of science, in the principal cities; that the circulation of the work is becoming extensive; and, that the publication will most assuredly be continued. If V. S. has any discovery in science or the arts to communicate, or any useful or important essays to publish, they will be inserted with great cheerfulness. But if he thinks the literary world is to be gratified with personalities, he ought to be content that he has found the *Medical Repository of New-York* a fit vehicle for them. If he has any more of the same complexion, we recommend him to the further patronage of the *polite* and *gentle* Editors of that work.

ERRATA.

Page 89, at the end of line 5 from the bottom, insert *up*.
96, line 9, for "and," read *at*.

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* A copy of this letter was sent to Dr. Miller, to be inserted in the Medical Repository which is to appear on the first of February.

MEDICAL AND PHILOSOPHICAL

Journal and Review.

VOL. II.—NO. 2.

DISSERTATION *on the Effects of a* JAVA POISON, *called the* UPAS; *on the Nux Vomica, the Faba Sancti Ignatii or Indica, the Strechnos Potatorum, and the Pomome de Vontac, Poisons of the same kind with the Upas; by Mr.* ALIRE RAFFENEAU DELILLE, *Member of the Egyptian Institute, and Doctor of Medicine of the Faculty of New-York.*

As these poisons were formerly used in medicine, and laid aside merely from the uncertainty and usual violence of their effects, while their success in the cure of some diseases remains equally upon record, we are indebted to Delille for the decisive experiments he has made on their powers, and the very clear manner in which he has proved their deleterious qualities.

The poisons were brought from Java by Mr. *Leschenault*, and from him we are promised a botanical account of the vegetables from which they are extracted, with specimens of the plants, and a true relation of the uses to which they are applied in the country. As these substances are of a very extraordinary activity, Mr. Leschenault gave Dr. Delille a certain quantity of them, in order to try the influence and degree of activity of certain vital properties of the animal organization.

A new motive engaged the attention of the Doctor to these inquiries; he sought to discover the fittest treatment for the disorders occasioned by these poisons, always known to be mortal if exhibited in sufficient quantity. Mr. *Magendie* assisted in the experiments, and a joint memoir was presented by him and the Doctor to the Institute, on the special action produced on the spinal marrow by the poison called in Java *Upas Tieuté*, and which is the inspissated juice of a parasitical vegetable of the family of the strychnos. They found that the same effects were produced by the resinous extract of the bitter seeds of plants of the same genus; such as the *Nux Vomica* and the *Faba St. Ignatii*. Then the means are pointed out to which it is proper to have recourse for counteracting the deleterious effects of these vegetables.

Mr. *Leschenault* acquaints us that the word *Upas* signifies a vegetable poison, and that the natives of Java employ two kinds. The first is the *Tieuté*, and this is furnished by a shrub; the second is the *Upas Antiar*, which comes from a large tree. These two kinds have been confounded by writers under the name of *Boa*, or *Bohon Upas*, and many fables propagated about its qualities. Both kinds are equally dangerous, but they act differently. The dissertation of which we now give an account, treats of the *Upas Tieuté;* but we are promised that the *Upas Antiar* will be made the subject of another work. The Upas Tieuté used in the experiments of Dr. Delille had been preserved in a close phial, and was of the consistence of a syrup. He and his colleague opened the integuments on the thighs of a dog, by two incisions of nearly six inches in length, (25 millimetres) and by a third incision on his neck, which penetrated into the substance of the muscles. The wounds bled very little, and were not licked by the dog. Several drops of the poison were inserted into them, but were not followed by any derangement of the animal's functions. A salutary degree of local inflammation appeared within twenty-four hours, and on the fifth day the wounds were healed. But this is not the mode of poisoning employed by the natives of Java and the other Islands. They suffer the poison to dry, and form a coating on the extremity of their darts. Our authors followed next the same process, and soon ascertained the prompt effects of the Upas when it has dried on the pointed extremity of a sharp body, and been inserted into the flesh.

A splinter of wood with two drops of the dried extract on it was stuck into a small opening, made by a scalpel into the thigh of a dog five months old, and weighing twelve pounds, (six kilogrammes). In four minutes the head became erect, the chest was raised upon the anterior extremities, which were the first to stiffen; the hind legs projected the body forward, and immediately the animal fell upon his face and breast, and then upon his side. The flexure of the spine backward, the extension of the limbs, and the general rigidity manifested a complete tetanus, in the midst of which the animal preserved the integrity of his sight and hearing; his limbs vibrated at intervals; convulsions seized him later in the face; the contractions of the heart were rapid and intermitting; every touch threw the body into agitation; the mouth foamed, the tongue and gums became blue. In the space of two minutes the animal died. In a quarter of an hour after he was opened. The poison had dissolved in the wound, and had given a brownish tinge to the muscular and cellular fibres. No morbid appearances were discovered in the abdomen, in the cranium, or in the medullary canal. The lungs exhibited the marks of asphyxy. The aorta contained black and fluid blood; the cavæ and pulmonary veins were distended with it; but the venous and arterial blood, which was effused, coagulated promptly.

These experiments were repeated on an older and robuster dog than the former. He was wounded with two grains of the Upas, and in three minutes exhibited marks of its effects. He would appear tranquil for a little while, and then his agitations would return at continually shortening intervals. In twelve minutes he experienced a complete attack of tetanus, in which the anterior extremities were the first to stiffen. The urine was involuntarily discharged. The least contact, or simply stamping on the floor, in the absence of the spasm, brought it back again; and thus you might, at will, renew contractions very similar to those produced by electricity or galvanism. Throughout, the animal preserved the integrity of his intellectual faculties. The phenomena which accompanied the attacks, whether spontaneous or brought on by contact, were a suspension of breathing at the height of the tetanus, a bluish lividness of those parts where the sanguineous vessels are distributed on the surface, such as the tongue and gums. In the intervals of the fits those parts resumed a colour approaching to their natural one. The halitus or expiration was charged with vapour, though the thermometer stood at 12, (59 Fahrenheit). The mouth was covered with a copious slaver, until at last death was brought on by repeated attacks of tetanus.

It clearly resulted, from a repetition of these experiments, that a very young animal died more quickly, and of a fewer number of attacks than an aged one. The size of the animal, the quantity of the poison infused, the disposition and properties of the various membranes to which the poison is applied, all greatly influence the duration and number of the attacks. The speediest death happened in four minutes, to a young spaniel, wounded with seven centigrammes (a grain and a half) of the Upas. It took place in a single attack, and began the third minute. The slowest death was that of a dog seven kilogrammes weight, (14 lb.) wounded with three centigrammes of Upas. It happened in one hour fifty-seven minutes, after an indeterminate number of attacks. In general, when the animal has been wounded, the effects commence within the first four minutes, or quarter of an hour, and death comes on ten or twenty-five minutes later, with more or less frequent attacks of tetanus. They are quite similar to those brought on by the irritation of sharp instruments, or galvanism, on the spinal marrow laid bare.

Effects of the Upas in the serous Cavities.

A few drops of Upas, diluted with a small quantity of water, were injected into the pleura of a dog weighing ten kilogrammes, (twenty pounds). The action of the poison was very sudden; the tetanus occurred in one minute, and in a minute and a half the animal died, as in the preceding experiments. The thorax was opened, but no particle of the injected fluid was found there; nor was there any mark of inflammation of the pleura. It was evident, however, that the poison must have been carried into the circulation. A similar injection, in a larger quantity, operated in the same way upon a horse, who died as suddenly by tetanus. The injections of Upas into the peritoneum produced the same effects as in the pleura, but more slowly, as if in proportion to the slower absorption in this cavity than the other.

Action of the Upas mixed with the circulating Blood.

A great many experiments were made to ascertain its power in this way. Eight drops of Upas were injected into the jugular of a horse, and they produced an immediate tetanus, and death. The effect was still more instantaneous on a dog. It follows from those observations, that if the Upas be communicated to the blood either by immediate contact, or by the absorption of wounds, or serous cavities, the invasion of the symptoms will be proportionate to the time requisite for the poison to be conveyed through the circulation to the spinal marrow. For it was determined that the symptoms depended on the spinal marrow, because on destroying it, the experimenters constantly found that the susceptibility of being acted upon by the Upas was lost to all the other organs; whereas the susceptibility remained if the spinal marrow was only divided between the occiput and the first vertebra.

It was proved that when the Upas mixed with the blood had a greater space to pass through before it could arrive at the spinal marrow than the distance from this to the jugular veins, the effect was also later in its occurrence. Having exposed the crural artery of a strong dog at its exit from the pelvis, and tied it in two places, a few drops of Upas were injected between the ligatures. After seven minutes the animal appeared disordered; the anterior extremities became stiffened, and he fell on his side. A general tension of the extremities and of the vertebral column followed, with an erection of the neck, and violent convulsions. The animal preserved his intelligence throughout, especially in the moments of remission from the violence of the convulsions, and when the mouth had resumed its natural colour, instead of the bluish livid hue that accompanied them, as already observed. A complete tetanus, an entire cessation of the respiration, a locking of the jaws, grinding of the teeth, marked the last moment of the animal's life. He suffered four minutes altogether.

The Upas introduced into the circulation of the brain by an injection into one of the carotids of a dog, occasioned an instantaneous subversion of the understanding, but did not produce tetanus before the poison had time to traverse the brain, be returned to the heart with the circulating blood, and thence reach the spinal marrow. Tetanus and suffocation induced death in six minutes. The extremity of the carotid discharged a dark coloured, instead of florid blood.

Effects of the Upas in the Stomach and Alimentary Canal.

Small doses of Upas, mixed with butter or meat, were given to dogs, some old, some young, and of different weights. They all died with the same series of phenomena as when the poison was communicated through wounds to the system. The only difference was as to the time in which absorption takes place through the mucous surfaces of the alimentary canal. The shortest period in which tetanus supervened was seven minutes; and the longest was an hour. The shortest course of the attack from the invasion of the symptoms to the death of the animal was eight minutes, and the longest one an hour and a half.

A great many dogs swallowed the Upas without dying of it;

but they all experienced some accidents proportioned to their age and strength, and the quantity they took of the poison. Where the quantity was very small they recovered; but, while suffering under the influence of the poison, touching them, making a noise, or shaking the floor, aggravated their symptoms.

In those cases where the Upas, taken into the stomach with the aliments, occasioned death, it produced more of the characteristic signs of poison, and there appeared no signs of inflammation.

It was found that where death was the most prompt, the Upas had passed with the aliments into the duodenum, but had not passed the pylorus, or only in very small quantity, where death was retarded. This is easily accounted for by the relative degree of absorption of the mucous membrane of the stomach, where it is slow, and of the intestines, where it is active. The fact and the difference were rendered more striking by the following experiments. The stomach was tied near the pylorus, and a few drops of Upas diluted with water were then thrown in by a small wound above the ligature, after which the wound was so closed as to prevent all communication with the cavity of the abdomen. In thirty-five minutes the animal began to tremble, and in twenty minutes more tetanus came on, and he perished.

In another dog a loop of the small intestines was taken out and secured by a double ligature, in the interval of which were injected, with the same precautions, and diluted with water, a few drops of Upas. In six minutes there came on tetanus; the animal sustained sixteen attacks, and died in four minutes more.

The Upas does not act by its application to a Nerve.

A deep incision was made in the thigh of a dog, and the sciatic nerve laid bare. It was then detached from the environing membranes for twenty-eight millimetres, (an inch) and supported by a little plate of lead. A few drops of Upas were then poured on the nerve, and insinuated also into its coat opened longitudinally. No inconvenience resulted but simply that of the wound, and this afterwards healed.

Experiments which afford a presumption of a direct Absorption by the Veins.

That the Upas is diffused, or its action communicated by means of the blood vessels from an insulated part to the rest of the body, is shown by the following fact. Between a part to which the Upas was applied and the rest of the body all communication was carefully cut off, except what was preserved by means of a single vein and a single artery. The nerves of the part were cut, and it remained immoveable; but the blood vessels were sufficient to convey the poison farther, and produce tetanus in the muscles beyond it. The experiment was carefully performed, by cutting the thigh of a dog close to the pelvis, and leaving out the trunks of the crural artery and vein, to communicate with the rest of the body. The trunk was seized with tetanus in ten minutes, and death supervened in a quarter of an hour later.

The same experiment was repeated on a stronger dog, with the same result. It was also repeated on a loop of the intestine, in which a vein and artery only of the mesenteric branches were preserved, and still with the same consequence.

The Action of the Upas may continue notwithstanding the division of the Spinal Marrow.

It was proved, by repeated trials, that animals affected with tetanus from the poison did not instantly become motionless on dividing the spinal marrow between the atlas and occiput, but that the convulsion was repeated two or three times, and that the poison continued to act until the last spark of vitality was extinguished.

It was ascertained that absorption could take place in the serous cavities, notwithstanding the solution of continuity in the spinal marrow. A dog of middling size had the medulla divided close below the occiput; the heart continued still to beat. Immediately some drops of Upas were injected into the pleura. In the space of a minute a convulsive stiffness seized the whole body. In twenty seconds more a new and violent stiffness came on. After the complete cessation of motion the animal was opened—there was no trace of inflammation found in the pleura, the lungs were unhurt, and the Upas was in part absorbed.

Effects of the Upas placed in immediate contact with the Spinal Marrow.

This was divided transversely between the first vertebra and the occipital. Five drops of diluted Upas were injected into the rachis (the medullary cavity), by means of a small syringe with a bent canula. No sooner did the liquor come in contact with the vertebral medulla than the fore-feet were stiffened, while the hind-feet remained still free and supple. In the space of a minute all the extremities were convulsed. In another instance the tetanus was made to begin in the posterior extremities, by bringing the poison first in contact with the lumbar portion of the medulla.

It was also proved by experiment, that the total destruction of the medulla spinalis was followed by an inability of being convulsed in all the muscles that received their nerves from it; and that a partial lesion was followed by a loss of motion in those muscles only that received their nerves from the part destroyed. A quantity of Upas was injected into the abdomen of a dog sufficient to produce a violent tetanus. The moment it commenced a slender whale-bone probe was pushed through the vertebral canal as far as the sixth or seventh dorsal vertebra. In that instant the tetanus ceased in the anterior extremities, and continued in the posterior ones: after which the whale-bone was pushed along to the sacrum, and then the tetanus of the posterior extremities ceased likewise.

Effects of the Upas on the Mucous Surfaces in general.

The Upas applied to the mucous surfaces of the rectum, the vagina, and the urinary bladder, constantly produced tetanus.

The diluted Upas applied to the conjunctiva produced tetanus, without inflammation, and with no mark but the colour of the poison. The tetanus produced by five or six drops of Upas upon the eyes killed dogs of eight or ten pounds weight, but did not do so with dogs of greater strength.

Six drops of Upas put upon the eyes of a dog somewhat inflamed produced tetanus much sooner than where the conjunctiva was sound.

Experiments instituted with a view to ascertain the best Means of remedying the Accidents produced by the Upas.

The most immediate and prominent danger from the Upas being suffocation, the first object was to seek for means for retarding its ingress, or removing it when begun, by renewing artificially the air in the lungs.

A young spaniel, weighing twelve pounds, was seized with violent tetanus after having been wounded with two grains of Upas five minutes before. He was in danger of suffocation at the first attack, in consequence of its duration and intensity. The tetanus having ceased, and respiration not returning, the animal was supposed to be dead. The trachea was immediately perforated, and the canula of a syringe inserted; on the side of it was a small hole that answered the purpose of a valve when covered with the fingers, and by means of this contrivance the atmospheric air was freely thrown into the lungs and returned. The artificial respiration thus carried on by means of the syringe restored the natural respiration in twenty or thirty seconds, and the animal lived in the midst of a protracted tetanus, kept up by the continued absorption of the poison. The attacks returned three times in twenty minutes, and in the interval the injection of air revived the animal. When the natural respiration took place again the canula was left in the trachea, as well to facilitate the passage of the air as to stop the flow of blood; but the respiration could not go on of itself when the tetanus returned, and for want of the working of the syringe all the signs of animal sensibility were suspended. Once more the mechanical respiration was restored, and the animal resuscitated. The injection of air was gently continued for a quarter of an hour, and the animal lived, though in a state of tetanus. He was then abandoned, and died in ten minutes. It is probable that he might have been kept longer alive if less time had been suffered to elapse without artificial breathing between the last attacks. Death had been retarded more than half an hour, if the time be compared in which this animal died with that in which other animals perished who had been wounded with an equal quantity of Upas, and not succoured with an artificial breathing.

The same experiment was repeated on another dog with similar results, and life protracted five quarters of an hour beyond the first invasion of the attacks.

These experiments gave an opportunity of making the following important observation: One of the crural arteries having been insulated and cut, and the blood commanded by means of a forceps, which held the upper extremity, it was seen that the injection of the air into the lungs coloured the arterial blood of a bright red in twenty seconds, and that the suspension of injection was followed, in an equal time, by a change of the arterial blood to black. It was ascertained above twenty different times, that as often as the air was brought, by means of the injections, in contact with the bronchial cells, the crural artery spouted blood of a bright red, and as often as the injections were interrupted the blood was dark coloured, like the venous. Nothing can prove more directly the influence of the air on the colour of the blood.

It remained to be ascertained, whether the artificial respiration continued sufficiently long, might not protract life until the action of the poison was exhausted, and the animal saved. It had been found in previous trials, that the poison in very small quantities did not produce death; but it equally appeared, that if the dose was somewhat increased it became mortal.

A new experiment was tried. A dog, of the same size as the preceding, was wounded with a point well covered with dried Upas, and which was withdrawn in a minute and a half. It was impossible to estimate correctly the quantity of the poison retained in the wound, but neither injection nor cauterization was employed to destroy or wash it away. For ten minutes the animal remained tranquil; but shortly afterwards he was seized with an attack of tetanus that endangered suffocation. The injections of air into the trachea were resorted to immediately, and they removed incipient suffocation and insensibility. These returned with every attack of tetanus, and were constantly overcome by a continuation of the injections of air for twenty-five minutes. The animal was then better; but for twenty minutes more felt occasional slight spasms, which went off of themselves, and were productive of no injury. The dog recovered.

From this experiment it may, at least, be concluded, that the artificial respiration was serviceable in preventing the suffocation and insensibility from reaching their highest pitch, and in aiding the animal to live during the action of the poison. He might perhaps be saved without it, as appears from the following case. A dog weighing eight kilogrammes (sixteen pounds) was wounded with three centigrammes (half a grain) of Upas. In an hour's time he had three attacks of tetanus; they went off in half an hour, without leaving any injury.

Many fruitless attempts were made to overcome the effects of the Upas with opium. Eight grains of the watery extract of opium were injected into the left side of the thorax of a dog. The irritation caused a momentary cough. In five minutes the animal fell fast asleep, and was not to be roused from his lethargy. He was then wounded with a grain and a half of Upas; in five minutes the effects of the poison were manifested by the presence of tetanus, which did not, however, remove the drowsiness. The jaws were locked, the neck grew tense, and the animal stiffened so as to remain standing. He jumped without awaking, fell, and had violent attacks of tetanus, that did not banish sleep. The wound was repeatedly washed without any benefit; for the space of an hour there were several agitations, during which a tolerably free respiration prolonged life. The lethargy at last diminished, and the animal died.

A small dog of five years old swallowed a bolus of four grains of opium and two of Upas; the opium produced a vomiting, that did not, however, entirely clear the stomach. Tetanus came on in thirty-one minutes, and the animal died like the preceding.

The American Indians are known to treat themselves for poisoned wounds with sea salt; and, according to the testimony of Mr. *Leschenault*, the same practice prevails among the Indians of the eastern islands. It was found by *Fontana*, that salt was nevertheless no specific for the American poisons, yet it may not be altogether useless; but, taken copiously, as it is by the Indians, and thence becoming purgative, it probably shortens the deleterious effects. These are not always fatal—the accidents being proportional to the doses of the poison.

A small dog had been made to swallow two grains of Upas, and two decogrammes (11 drs. 20 grs.) of muriate of soda mixed with molasses. The salt was immediately thrown up, and the pill partially dissolved. In a quarter of an hour the animal vomited thrice a mixture of blood and phlegm; in five minutes more he was purged. In three quarters of an hour from the time of his swallowing the poison and salt his limbs stiffened, and he had several loose stools. After three slighter attacks there came on a more violent and continued tetanus, which proved fatal in three minutes. On opening the abdomen the small intestines were found much inflamed; the stomach was full of blood, and some coagula appeared in the duodenum.

Dr. *Delille* and Mr. *Magendie* repeated all those experiments at different times, and do not twice quote the same, but when they were attended with some difference in the dose of the Upas, the manner of employing it, or the duration and termination of the symptoms.

Having wounded a dog with a grain of Upas, and then immediately conveying into the stomach, by means of an elastic tube, a solution of muriate of soda, vomiting, tetanus, and purging came on, as in the former experiment, but no blood was thrown up, and life was more rapidly extinguished, owing, no doubt, to the Upas remaining in the wound.

We have the following results:

Animals did not suffer from the wounds of the poisoned arrows, if these were immediately withdrawn. When the arrows were barbed, and the extraction of them slow, the consequences seemed to be proportionate to the quantity of poison dissolved and absorbed.

The effects of the Upas may, in many cases, be prevented. It poisons only through the circulation. A hemorrhage from the poisoned part, by washing away the Upas, prevented the poisoning; a ligature above the wounded part prevented, in like manner, the effects of the poison. In one instance no bad consequence was observed for twenty minutes after the insertion of the Upas, but in one minute after the ligature was removed tetanus supervened. The ligature was quickly replaced, and the tetanus gradually subsided; it was again removed, and so soon the animal was carried off by a violent attack of tetanus.

The best remedy when a wound has been inflicted is, immediately to withdraw the instrument, and sear the part.

Wherever a ligature may be applied it should be instantly performed, and not by any means removed until the wound has been cauterized, and some blood evacuated by scarifications.

If taken into the stomach, the Upas should be instantly discharged by emetics.

The danger is always relative to the age and strength of the patient, to the quantity of the Upas communicated, the time it remained in the wound, or the greater or less absorption of the surface to which it is applied. Thus a very little delay in the serous cavities is without remedy, and the danger is greater when the poison gets into the intestines than when it is confined to the stomach.

Bleeding might do some little service in retarding the quickness of the circulation, and the approaches of suffocation. Purgatives used after the example of the Indians might also cause some delay or omission in the evasion of the attacks.

The Upas poured on the wound of a dog produced no inconvenience; nor on that of a rabbit, an animal that dies more readily, unless where it is laid on in large quantity, and in a manner stuffed under the skin.

Wherever there is a large opening, by which the poison can run off, there are no bad consequences; but where the poison is inserted, as in the case of being detained in the cellular tissue, its usual effects are soon manifested.

Dr. *Delille* and Mr. *Magendie*, whose hands were scratched by the animals they opened, experienced no inconvenience, no inflammation in those parts from the contact of the Upas.

The poisoned arrows, whether they penetrated to the cellular tissue only, or the muscles also, near the large blood vessels, or nerves, produced the same accidents.

If the wound be gaping, and that there is much hemorrhage, or a coagulum formed round the point, there is comparatively little danger. Hence a fine pointed arrow, round which the skin closes tight, gives the most dangerous wound.

In proof of this, a strong horse was wounded in the ham by a sharp splinter, one inch and an half broad (twenty-five millimetres), and covered with eight decigrammes (fifteen grains) of Upas. The end was broken in the wound. A torn artery caused an hemorrhage, which compresses could not entirely stop. It continued an hour and a half without any sensible injury having appeared. The wound was then opened and the splinter withdrawn. It was surrounded with a coagulum, the wound was washed, and the poison inoperative. Another splinter, of the diameter of a crow's quill, and three inches long, was covered with fifteen grains of Upas, and, through a small opening in the skin, plunged deep into the muscles of a horse's thigh, where it was broken, and left behind. Tetanus came on in half an hour, and in seventeen minutes more he died of the third attack.

The flesh of animals killed by the Upas acquires no bad quality from it, except in those parts to which the poison adheres. The dogs that devoured the horses killed by it suffered no inconvenience; but dogs that eat the stomach and intestines of other dogs that had taken it internally were affected in proportion to the quantity that remained on the part, while the flesh of the carcase was devoured by other dogs with perfect impunity.

The Indians, according to the relation of Mr. *Leschenault*, eat the flesh of the animals they have killed with poisoned arrows, but first throw away the parts that have been wounded.

Comparative Experiments, showing the Action of the Upas, and that of several kinds of Strychnos.

Messrs. *Delille* and *Magendie*, wishing to compare the action of the Upas with that of other poisons, chose them of the genus to which they judged it almost certain that the Upas belonged. Though Mr. *Leschenault* brought from Java but a small branch, and the leaves of the Upas plant, the rules of botany, according to the disposition of the leaves alone, rendered it highly probable that the plant belonged to a class of poisons, and Mr. Jussieu referred it to the Strychnos.

Experiments with the Nux Vomica.

This poison was taken such as it is found in the druggists' shops, grated, and in small fragments.

The first trial was made on two dogs of nearly the same age and size, with half a drachm of the grated Nux Vomica to each. The first was seized with tetanus forty-two minutes after having swallowed the poison. The limbs were spread, some convulsive starts threw the animal forward, and the neck stiffened. The anterior extremities were affected with tetanus in fifty minutes, and the posterior extremities shortly after. Three attacks were felt in succession; there was a discharge of urine, integrity of the intellectual faculties for some time, change of colour at the mouth, froth, suffocation and insensibility, death.

The opening of the body exhibited the usual symptoms of asphyxy, namely, the passage of dark blood into the arterial cavities.

One of the dogs died more slowly than the other, and in the former the Nux Vomica remained in the stomach, in the latter it had passed into the duodenum.

No trace of inflammation was found in the intestinal canal of the dogs poisoned either by the Upas or Nux Vomica; nor perhaps should any be looked for from poisons whose action is so rapid.

It is common enough to find on those dogs a little redness about the pylorus, and to see some longitudinal red stripes on the duodenum; but these marks exist in very different cases, and are no certain criterion of inflammation.

The cerebral and medullary cavities do not show after death any trace of the action of the Nux Vomica.

Small doses of the Nux Vomica produce slow and numerous attacks of tetanus. In very small doses its effects are slight and temporary.

Experiments with the extract of the Nux Vomica.

The similitude of the poisoning of the Nux Vomica and the Upas, when taken into the stomach, being ascertained, trial was next made of its operation in the serous cavities. For this purpose twelve grains of the extract, dissolved in two drachms of water, were injected into the pleura of a dog. In one minute they produced tetanus, and, notwithstanding the division of the medulla immediately under the occiput, two other attacks returned before death.

If the liquid extract be put on wounds it is not hurtful. This was repeatedly tried, and it was found that, like the liquid Upas, it only acts when buried between the muscles, or inclosed under the skin by injections.

The dried extract acts on wounds like the Upas, but requires to be applied in larger doses, because it is less resinous, and that the resin is the most active part.

The resinous extract of the Nux Vomica, prepared by the maceration of this seed in spirit of wine, after being entirely separated from the guminous part, by repeated ablution in pure water, is equally active as the Upas of Java. This was proved by the equal quickness with which it killed dogs and rabbits.

All that has been said of the effects of the Upas, and the means of lessening its danger, is applicable to the Nux Vomica. The asphyxy it brings on in consequence of the immobility of the chest during the tetanus may be overcome through means of an artificial respiration, unless the dose be so great that its action cannot be exhausted. In a dog severely poisoned, but kept alive after this manner, the tetanus had so far diminished at the end of an hour, that respiration continued of itself, and the animal survived.

If vomiting be excited within a short time of swallowing the poison, its power of injuring ceases.

A ligature put soon enough above a poisoned wound prevents the effects of the poison, and there is no danger if it be withdrawn from the wound before it mixes with the blood.

Whenever the Nux Vomica, in powder, or in extract, was given in so small quantity as not to cause tetanus, it did not prove dangerous; or if so slight as not to prove mortal, no other bad consequences followed.

The Nux Vomica then produces effects which we may regulate, when it is given in very small doses. By means of the absorbent and sanguiferous vessels it acts on the medulla oblongata, and moves those muscles to which this sends nerves. In case of an atony of the medulla the Nux Vomica might perhaps be of service.

The symptoms produced by the Nux Vomica are well described by Murray, from Loss. The latter has observed, that it does not induce sleep like the true narcotics; and the same fact was confirmed by the experiments of Dr. Delille. Loss found that pigs, goats, and hens could take large quantities of it without inconvenience. He also observed that it leaves no trace of inflammation in the stomach.

Dr. *Roxburg* relates that they distil the Nux Vomica in the East-Indies with spirituous liquors, in order to render those more intoxicating.

At Cairo, and in Persia it is an ingredient of intoxicating electuaries.

The difference of shape between the Nux Vomica, which is regular, and the *Faba St. Ignatii*, which is more or less angular, makes it easy to distinguish those seeds, improperly called Nut and bean. The analogy of taste discovers in them the same properties, and the trees on which they grow have in their flowers the same number and arrangement of parts, and among their leaves the same disposition. Jussieu classes them in the same genus. The poisonous qualities of the *Fabi Sancti Ignatii* are the same as those of the Nux Vomica, and Dr. Delille subjected it to the same series of experiments as those already related. This species of Strychnos, or *Nightshade*, is remarkable for intense bitterness.

Another species of *Strychnos*, called in India *Tetam Cotté*, and by Linnæus the younger, *Strychnos Potatorum*, has no bitter taste, but is very mucilaginous. According to *Roxburg*, the grains of this Nightshade are rubbed against the sides of the vessels holding muddy water. The process is the same as that used in Egypt with the common almonds—the mucilage causes a precipitation of the suspended slime, and the water remains clear. Another kind of Strychnos is the *Pomome de Vontac*, which has no bitterness, no more than the former.

The experiments made with three kinds of Strychnos that are bitter, and two that are not, show how plants of the same genus, and possessing the same sensible qualities, have also the same properties; and that several species of the same genus, not possessing the same sensible qualities, have no more similitude in their powers. This is further manifested in other plants naturally classed together, such as the arum, vanilla, pines and palms, whose properties differ according to the juices with which they are impregnated.

We have been thus full in our account of Dr. Delille's experiments, from the importance of the subject, and the light they throw upon some interesting particulars of the animal economy. The accuracy too with which they were performed renders it unnecessary to repeat such cruel inquiries, and, of course, it was the more incumbent on us to detail them at large.

From the Annual Medical Review and Register for 1809.

FACTS *establishing the Efficacy of* OPIATE FRICTION *in* SPASMODIC *and* FEBRILE DISEASES. *Also Outlines of an Attempt to investigate the Nature, Causes, and Method of Cure of Hydrophobia and Tetanus; republished from the London Medical and Physical, and the Edinburgh Medical and Surgical Journals. To which are added, Cases and Remarks not before published. By* MICHAEL WARD, *late Surgeon to the Manchester Infirmary, Dispensary, &c.* 8vo. pp. 208. *Printed at Manchester.* Bickerstaff, *London.*

WE have been so long acquainted with the fact, that the operation of active medicines may be obtained through the medium of the cutaneous absorbents, with a certainty almost equal to that of their operation on the stomach; and with regard to one of them, mercury, we have been so constantly in the practice of resorting to it, that nothing but the inveterate influence of habit can be alleged, in explanation of the tardiness with which we have applied our knowledge of the fact to other powerful remedies. Foreigners, and especially the physicians of Italy, have anticipated us considerably in their observations upon this subject; but some of them, from hypothetical views, seem to have been misled into an opinion, that certain animal fluids were requisite, as solvents, to effect the absorption of the substances employed. Thus, Dr. Chiarenti applied opium externally, dissolved in gastric juice, and afterwards squill and digitalis in the same manner; but, from the difficulty of procuring this fluid in sufficient quantity, he was induced to substitute saliva, which afforded similar success. Several other Italian physicians adopted this method. Professor Brera, of Pavia, in a work published in 1797,* relates many cases, in which the medicines above mentioned, as well as tartar emetic, aconite, corrosive sublimate, &c. were successfully administered by external friction, suspended in saliva; whence, he concludes, " that every animalized fluid is fitted by nature to render remedies capable of being absorbed." In the the same year, however, Dr. Chiarugi, of Florence, published a letter, " Sul' Uso Esterno del' Oppio,"* in order to prove that all the effects of this medicine might be produced upon the animal economy by external friction, without the aid of those animal fluids; and that, by this mode of exhibiting opium, the paroxysms of mania had been speedily removed in several cases, and in some a complete cure had been effected.

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* See a brief account of this work in Duncan's Annals of Medicine for 1798.

It was from this statement of Dr. Chiarugi that Mr. Ward was first induced to employ an opiate in unction in the delirium of typhoid fever; and the success which he experienced led to the application of the same remedy in other diseases, and to the suggestions, which he now offers to the profession, in regard to its use in some other diseases of difficult management, or which defy, as yet, all the efforts of our art.

We cannot refuse our tribute of approbation to this attempt of the author, to extend our information on a subject too much neglected; as well as to the candid manner in which he has apparently related his facts, by acknowledging occasionally his want of success, and admitting to their full share of efficacy the other remedies employed in combination. The necessity of the re-publication of the facts, scattered through several journals, is evinced by the oversight or inattention with which they have been received: together, they constitute a body of evidence which, we trust, will be instrumental in recommending the practice to farther experiment. It is true, indeed, that in a great majority of cases, in which opiates are indicated, the more speedy and certain operation of the medicine, and the more accurate knowledge which we possess of the dose, when given by the stomach, must render the troublesome mode of external friction unnecessary and ineligible: but there are not a few circumstances, under which it may become the preferable, if not the only possible mode of conveying opium into the system. As we avoid the most objectionable and hurtful effects of mercury, viz. its influence on the alimentary canal, by administering it through the medium of the cutaneous absorbents; so it would appear, from the observations of our author, seconded by those of the late Doctor Percival, that those effects of opium, which constitute the principal objections to its use, in particular constitutions and in certain states of disease, are generally avoided by its external employment. Mr. Ward observes,

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* See a brief account of this work in Duncan's Annals of Medicine for 1798.

> ‘ From the facts above recited, I think I am warranted in drawing the following inferences: 1st, That opium, when diligently applied externally, so as to be absorbed by the lymphatics, has powerful effects in allaying irritation, removing spasm, and procuring sleep. 2dly, That it is capable of producing these happy effects, where the exhibition of it internally had not the same salutary operation. 3dly, That this mode of introducing it into the system may be resorted to with advantage, when it cannot be given internally, or when it will not stay on the stomach.’ p. 16.

Dr. Percival, who tried the opiate friction in consequence of the suggestion of Mr. Ward, informed him, that, since a former communication he had employed it ‘ in a chronic dysury, and in a case of stone in the bladder, with very considerable ease to the patients, and without producing the vertigo, head-ache, and obstinate contiveness, which the internal use of laudanum had before occasioned.’ p. 26. In some other cases, the same absence of the usual effects, especially the constipation arising from the internal exhibition of opium, is noticed; and, in others, excessive vomiting and irritability speedily yielded to the friction. Two cases of typhoid fever with delirium, five or six cases of mania, one of delirium connected with a fractured limb, two cases of gangrene, and cases of vomiting, hiccup, hooping-cough, cholera, &c. are described as successfully treated by the external application of opium.

There are some cases, as we have just remarked, in which the internal use of opium is absolutely impracticable: such are those cases of trismus, whether of a tetanic or hysterical nature, in which the jaw is so firmly locked, the teeth at the same time being close and regular, that nothing can be made to pass to the œsophagus. In an instance of this kind, which occurred several years ago, in an hysterical female, we employed friction on the epigastrium, with a liniment of opium, camphor and oil, with decided relief, but not with the complete effect described by Mr. Ward. The friction, however, was not well managed by the attendants. Several cases of tetanic affection, and of hysteria, are here detailed, in which the opiate friction, especially when aided (in the tetanic diseases) by cold affusion, produced a favourable termination.

From a consideration of the success of opium, externally exhibited, in tetanus, which bears some analogy to hydrophobia, and of the peculiarly irritable condition of the pharynx and œsophagus in the latter disease, Mr. Ward was induced to suggest the propriety of refraining from the use of all internal medicines, in this hitherto uncured disorder, and of trusting to the free external administration of opium. Thus far we accompany the author with much satisfaction; but when he enters into the field of speculation, he mounts a courser which is too fleet for our ambling intellects, and leaves us beating about for analogies which may afford us some object of pursuit: we know not whether Darwin himself, on his philosophical Pegasus, could keep pace with him. After stating, that 'hydrophobia and tetanus belong to the same nosological class and order,' (thus referring to artificial and arbitrary associations, instead of describing the points of resemblance in the two diseases,) and informing us that 'the remote causes are various, according as the diseases are idiophatic or symptomatic,' though we know of no other remote cause of the *rabies canina*, (which is understood, we presume, by the terms 'idiophatic hydrophobia,') save the bite of a rabid animal,—our author thus proceeds :

> "The predisposing cause of both these diseases consists in an unequal and irregular distribution of the nervous or sensorial power; the tendency to which is increased by external heat, debility in the muscular and other coats of the stomach and intestines, and of the involuntary muscles generally, impure air, &c.
>
> "The proximate cause consists in *an exuberance and retrograde action of the faculties of the sensorium affecting the voluntary muscles.*
>
> "The proximate effect is the disease itself, which consists in *a spasmodic and retrograde motion of the fibres of the voluntary muscles.*
>
> "*In violent tetanus every voluntary muscle is thus affected.*
>
> "*In hydrophobia, the spasmodic and retrograde motions are principally confined to the pharynx, the œsophagus,*" (are these voluntary muscles?) "*the epiglottis, the tongue, the muscles employed in deglutition,*" &c. p. 109 et seq.

Lest the peculiar points of excellence in his hypothesis should be overlooked, he makes this abundant display of Italics. Admitting these definitions, we must, of course, maintain that tetanus, chorea, hysteria, hydrophobia, puerperal convulsions, and other spasmodic diseases, differ from each other only in the difference of the muscles affected, notwithstanding the difference in the exciting causes, in the constitution, age, and other circumstances of the individuals usually attacked, and in the various effects of remedies, as well as in the variety of the duration and modes of termination of the diseases respectively. We do not presume to understand the "retrograde motions" of a voluntary muscle; since we know of no other motion of which it is capable, except that of contraction, the cessation of which is relaxation. Even in an involuntary hollow muscle, what has been called a retrograde action, is only a repetition of the usual contraction of its separate fibres, succeeding in a series the opposite to that of the ordinary succession. However, the author boldly proceeds to his practical inferences.

"Thus it appears, that three different states of the nervous power subsist *at the same time; namely, an exuberance, a deficiency, and a retrograde action. The voluntary muscles are the seat of the first and third, and the involuntary of the second,"* &c. p. 116: and, of course, "the indications of cure, according to this view of the subject, will be clear and simple; viz. 1st, *to restore the balance in the distribution of the nervous or sensorial power;* and, 2dly, *the natural, that is, the* PROGRESSIVE *action of the muscular fibres."* p. 118.

Simple enough, in good truth! How long will medical writers persist in mistaking a change of terms for a new view of facts? What is here indicated by the words, "to restore the balance," &c. but "to restore the healthy function of the muscles?" in other words, "to cure the disease!" Our author, however, does arrive at a more specific indication; namely, that, in order to produce the wished for restoration, "the superfluous or exuberent portion of the nervous power must be repelled *from the voluntary to the involuntary muscles, i. e.* from the circumference to the centre:" hence, it is very clear, that "*no medicine, or combination of medicines, administered internally, can be adequate to the production of these effects.*" Q. E. D. See p. 118.

Now, we profess ourselves to be among those who conceive the most successful operations of our art to be incapable of satisfactory explanation, such as the treatment of syphilis by mercury, and of intermittent diseases by bark and arsenic; and, therefore, we should have valued a single fact, or even a brief analogical statement, above all this tissue of fanciful and speculative verbiage, from which we are not a whit the more disposed to expect success from the expedient here recommended. Considering hydrophobia as a disease hitherto eluding all the means of relief that have been pursued, and as consisting principally of the most acute and torturing augmentation of sensibility and irritability, which the human body can exhibit, we should be led to try the effects of the most powerful narcotic in its full extent. From this view of the subject, it was suggested, by a very intelligent medical friend, some time ago, that a *bath* of laudanum might possibly afford relief in hydrophobia; and, from the operation of opium, externally applied, as described by Mr. Ward, we should be more than ever disposed to anticipate advantage from such an expedient. But our expectations must be considerably impaired, on the other hand, by the recollection of the large quantities of opium administered internally, by Dr. Babington and Dr. Wavell, in two cases of hydrophobia, without producing the smallest benefit.* In one of these Dr. Babington administered no less than *one hundred and eighty* grains of solid opium in the short space of twelve hours; in the other *fifty* grains were exhibited within twenty-four hours, besides *ten* in an enema, and *five* more by the mouth within an hour afterwards, without producing any apparent effect. These facts ought to remove Mr. Ward's supposition, founded on no appearance of evidence or probability, that opium may be an antidote to the canine poison; and the unsuccessful case, in which opium was externally employed by Dr. Bardsley,† is rather adverse to his sanguine hopes of the efficacy of his remedy.

Our readers must have had a sufficient portion of Mr. Ward's hypothetical lucubrations; and, therefore, we shall omit any account of his speculation relative to the "*spasmodic* disease," diabetes, and its analogy to hydrophobia and tetanus; which analogy we conceive to be purely adventitious; namely, that the disease stands, whimsically enough, in the same order with them in Dr. Cullen's *Nosology*. We shall confine our few remaining observations to the modes of employing the opiate friction which our author recommends.

An ointment composed of one part of opium, finely powdered, with four or five parts of axunge, and one or two of olive oil, is esteemed an adequate application in most instances: it does not appear, however, that any great nicety is requisite in the proportions, provided the opium be in sufficiently fine powder before it is mixed with the other ingredients. Mr. W. adds, that "the smaller the quantity of unguent the better, provided the whole be made so smooth and soft as not to occasion any uneasiness by its tenacity in applying the necessary degree of friction." The quantity of opium which he usually applied at each time was from six to nine or ten grains, with about three drachms of hog's-lard. Dr. Percival employed the following formula:

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* See Medical Records and Researches, Art. viii. p. 117.

† Med. and Phys. Journal, vol. xiii. p. 155.

℞ Opii purif. grana decem
Unguenti Adipis Suillæ unciam dimidiam
Camphoræ grana quinque. M.

This quantity was rubbed in at each time. Dr. P. likewise used the tincture of opium (three drachms), formed into a liniment, with an equal quantity of olive oil, by means of the yolk of an egg. "But the tincture alone," Mr. W. remarks, "rubbed into the skin of the arms, has produced more speedy and powerful effects than any other preparation I have tried." p. 28. He directs "from three to six drachms, according to the age of the patient, and the severity of the attack," (he is here speaking of hysteria), "of the tinctura opii to be rubbed upon the insides of the arms, between the wrist and shoulder, until the whole is absorbed; which has commonly been succeeded, in the course of a quarter or half an hour, sometimes sooner, by a remission of the spasms, and composure of body and mind."

Several of the papers, which are re-printed from the journals mentioned in the title-page, are communications from other practitioners, who had been induced to try the mode of practice which Mr. Ward had recommended. We think, however, that, with very little additional industry, Mr. W. might have arranged the whole in a more lucid order, and have condensed his own papers, so as to have placed the facts in a more satisfactory light, and to have avoided a considerable degree of repetition.

From the same.

Philosophical Transactions of the Royal Society of London, for the Year 1809.

IN the first part of the Philosophical Transactions for the year 1809 there are several papers which will require some notice; but those to which we shall principally direct our readers' attention are Dr. Young's inquiry into the functions of the heart and arteries, and Mr. Davy's continuation of his analytical researches.

The principal design of Dr. Young's paper is to show, that the circulation of the blood through the arteries is but little promoted by their muscular power. He commences by the position, that " the mechanical motions which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies." This he illustrates by the effect of gravity, and by the refractive power of the eye; and he concludes, that " there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process calculated to overcome those affections by others, which are commensurate to them, and which are of a contrary tendency." The application of this conclusion to the present subject is obvious; the motion of the blood is to be considered as that of a fluid, which obeys the laws of other fluids placed in the same circumstances; and, therefore, the solution of the different problems respecting the progressive motion of the blood, as depending upon the muscular or elastic powers of the arteries, becomes a question in hydraulics, to be resolved by the acknowledged laws of this science, provided only that we are able distinctly to appreciate the nature of these powers.

In the prosecution of his subject, the author proposes to himself four distinct heads of inquiry:

"1st. What would be the nature of the circulation of the blood if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone: in the second place, in what manner the pulse would be transmitted from the heart through the arteries if they were merely elastic tubes; and, in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall, lastly," he continues, "add some observations on the disturbances of these motions, which may be supposed to occur in different kinds of inflammations and of fevers."

The blood, while in the arteries is subjected to a certain degree of pressure, by means of which it is forced into the veins; and it is supposed, that this pressure is almost entirely employed in overcoming the friction of the vessels. It becomes, therefore, necessary to inquire into the amount of the pressure, and the resistance which is produced by friction, or by any other cause of retardation. To ascertain these points, the author has recourse to the experiments of Hales, to the calculations of Keill and Haller, and to the theorems which he formerly deduced from his experiments on capillary tubes.

In considering the nature of the propagation of the pulse, Dr. Young observes, that the transmission of the wave of blood through the artery, is so analogous to the motion of waves on the surface of water, or to that of sound transmitted through air, that the same calculations may be employed in all these three kinds of motion. Proceeding upon his second supposition, that the coats of the arteries are perfectly elastic, he again recurs to the experiments of Hales, and deduces from them the velocity of the pulse, and the dilatation which it occasions in the vessels. It appears that the increase of size in the aorta will be about one-sixteenth of its bulk, and that the blood moves along it at the rate of fifteen and a half feet in a second. Proceeding upon these data, and also upon the principle, that the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of their joint areas, which is analogous to what takes place with respect to the decrease of the intensity of sound, it is estimated, that in very small arteries, the dilatation will not exceed the one hundred and sixtieth of their diameter, which will be a quantity too small to be observable by the human eye, even aided by the microscope. Inconsiderable, however, as this quantity appears, there is reason to suppose, that when the parts are in their natural state, the velocity of the pulse is greater, and the dilatation consequently less, than in the above ratio.

With respect to his third object of inquiry, the functions of the muscular coats of the arteries, Dr. Young, from considering the small amount of the dilatation of the vessels, is led to conclude, that they possess little power in the propulsion of the blood. He is farther induced to form this opinion from the great velocity of the blood; for it is obvious, that if it be pushed forwards by the contraction of the muscular fibres, the successive contractions of the different portions must advance as rapidly as the pulse, which is the effect of the contraction. We doubt, however, whether this be any real objection: for although we may experience some difficulty in the conception of this rapidity of action, it affords no ground for supposing that there is any difficulty in the thing itself. Although, as it appears, our author supports the opinion, that the muscularity of the arteries has but little effect on the propulsion of the blood through them; yet he candidly states, that there are some facts, which may seem to lead to an opposite conclusion. There are cases on record of monstrous births, which have been produced without hearts; and in many of the lower tribes of animals, this organ is always wanting. We do not think that any objection can be fairly derived from the structure of the inferior animals; because in them the aorta performs the office of the heart; and, with respect to monstrous productions, we can scarcely argue from them to what takes place in ordinary circumstances; for we often find, that where any organ is defective, the functions of some other parts undergo a change, in order to adapt themselves to the particular circumstances of the individual. If, therefore, Dr. Young's principle be well established on other grounds, we should not think it impossible to reconcile it to any cases of unusual formation. The author conceives, that the principal use of the muscular coat of the artery is to enable the vessel to accommodate itself to the variable quantity of its contents.

Taking it for granted, that the dilatation of the artery is inconsiderable, Dr. Young proceeds to inquire into the causes which produce an effect upon the state of the pulse, as felt by the finger. It must chiefly depend upon the action of the heart, and upon the tension of the vessel, owing to the quantity of blood which it contains. When the artery is much distended, and the force of the heart is great, the action of the heart will be rendered more perceptible to the touch, and a throbbing will be felt, like that in aneurism. On the contrary, when the pulse is small and feeble, it must be owing to the permanent contraction of the artery, and the weak action of the heart.

In the prosecution of his inquiry, Dr. Young has exhibited a great extent of knowledge, and very considerable ingenuity; yet we feel scarcely prepared to follow him in all his conclusions. The paper, however, deserves to be very attentively studied by the physiologist, although the result should be a partial dissent from the author's peculiar doctrines.*

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* However ingeniously Dr. Young may argue, that his conclusions are erroneous, is demonstrated by the following account of a dissection, related by Burns in his work on the Diseases of the heart, p. 131, 132.

"On opening the thorax, the pericardium appeared at first sight healthy, but it was enormously loaded with fat. By more close inspection, however, independent of manual examination, we soon discovered an obvious deviation from the usual colour and structure of the part. Instead of the ligamentous membranous appearance peculiar to the pericardium, it had assumed an opaque and a dusky hue; ragged spiculæ of bone likewise projected from its surface, rendering it very unequal. It adhered with the greatest tenacity to the parts in the vicinity, and where it invested the ventricles, it appeared incorporated with the substance of the heart. The whole extent of the pericardium covering the ventricles, *and the ventricles themselves, except about a cubic inch at the apex of the heart, were ossified and firm as the skull.* Not only the pericardium but also *the fleshy parietes of the ventricles, and some of the columnæ carneæ were changed into solid bone.* The ossified part formed a broad belt round the ventricles of the heart." In this case then the circulation must have been carried on by the muscular coats of the arteries solely, and yet the pulse was regular until within a day or two of the death of the patient, when, for the first time, mention is made of its feebleness. *Ed.*

Mr. Davy still continues his brilliant career of discovery, by the application of the Galvanic power to the decomposition of those bodies which were formerly thought to be elementary. His first object was to prove, in a more decisive manner than he had hitherto done, the presence of oxygen in ammonia, and thus establish a complete analogy between the volatile and the fixed alkalies. By causing potassium and ammonia to act upon each other, the ammonia disappears, hydrogen is evolved, and the potassium is converted into an olive-coloured mass, which appears to consist of this metal in union with the greatest part of the ammonia. During this process, and afterwards, while the compound is subjected to heat, both hydrogen and nitrogen are evolved, and there is a loss of hydrogen: at least, less is obtained by this means than when ammonia is decomposed by the action of common electricity. The author proposes several conjectures for the purpose of explaining this apparent contradiction; but it appears to be a subject on which we are, at present, unable to offer any opinion that is entirely satisfactory. The general conclusion that may be drawn from these experiments is in favour of the presence of oxygen in ammonia; but we cannot consider the composition of this substance to be as clearly ascertained as that of the other alkalies.

The author's next experiments were on sulphur. A strong electrical power was passed through this substance, when raised to a high temperature, and a quantity of sulphurated hydrogen was procured; and the same substance was also extricated by the action of sulphur on potassium. From the phenomena which attend this process, there is reason to suppose, that oxygen is present in the sulphurated hydrogen, and that it was previously contained in the sulphur. The conclusion that we draw from these experiments is, that sulphur consists of a small quantity of oxygen and hydrogen, united to a quantity of some peculiar base. Analogous experiments were performed on phosphorus, and generally with the same result, although the effects were scarcely so decisive. The different carbonaceous bodies, plumbago, charcoal, and the diamond, were next examined: by heating them with potassium, and observing the results, we are led to conclude, that in plumbago the carbon exists merely in combination with iron, being itself in a state of considerable purity;—that charcoal contains a little hydrogen, and diamond probably a minute quantity of oxygen.

The three acids hitherto undecomposed, the boracic, the fluoric, and the muriatic, were each in succession made the subject of a series of experiments. The boracic acid, by being heated with potassium, seems to be decomposed; the potassium is oxidated, and a substance is mixed with it, probably of a metallic nature, which may be regarded as the basis of the boracic acid. The quantity of oxygen which enters into its composition, is probably considerable, constituting two-thirds of its whole weight. By the action of potassium on fluoric acid gas, a similar kind of decomposition appeared to be effected, although the separate existence of the fluoric basis was less obvious. The decomposition of the muriatic acid Mr. Davy has not yet been able to accomplish, in consequence of the impossibility of obtaining it entirely deprived of water. He gives an account of some very ingenious expedients which he practised, in order to obtain dry muriatic acid; such as forming compounds of it with sulphur and phosphorus, and subliming the compounds. His ingenuity has not, however, yet been crowned with the usual success. We may, in general, remark upon this paper, that if it appear less curious and interesting than some of those which have preceded it, we must ascribe it to the nature of the subject, which does not admit of those striking and decisive results which had been obtained in the former instances.

We shall briefly announce the subjects of the remaining papers in this volume. Mr. Brodie relates a case of a fœtus that was born in a state of considerable maturity, in which the heart was entirely wanting. The absence of the heart was not the only mal-conformation; the stomach had no cardiac orifice, there was properly no colon, nor was there any omentum, liver or gall-blander. The umbilicus had one artery and one vein, and the artery was carried to the left groin. The only communication which existed between the arteries and the veins was by means of the capillaries, and of course the circulation must have been entirely carried on by the arteries. We are rather surprised that the author should not have made a particular examination of the large arteries, to observe whether they exhibited any unusual degree of muscularity, or whether any part of them was dilated, so as to form a reservoir, that might, in some degree, compensate for the want of a heart.

Mr. Knight, on a former occasion, had brought forward the opinion, that the buds of trees originate from the alburnum: in the present volume he endeavours " to show, that the roots of trees are always generated by the vessels which pass from the cotyledons of the seed, and from the leaves, through the leaf-stalks and the bark, and that they never, under any circumstances, spring immediately from the alburnum." The author conceives, contrary to the common opinion, that the radicle of the seed is not the rudiment of the future root of the plant, a supposition founded upon what he imagines to be a general law of the vegetable economy, that a proper root is always lengthened by the addition of successive portions of new matter to its apex, while, on the other hand, a shoot is enlarged by the addition of new matter within the previously organized substance. Now, it appears, that the radicle grows by the latter of these processes, and, therefore, cannot be regarded as analogous to a proper root. In order to prove his position respecting the formation of the root, the author adduces the following considerations. The proper root begins to be formed before any alburnum exists, but at a period when the cortical vessels are full of sap; the leaf-stalks of many plants can produce roots, although they are destitute of alburnum; roots are also readily formed from a shoot, the bark of which has been divided; and here it is observed, that the root proceeds from the upper lip of the wound, as if owing its existence to the obstruction which takes place to the descent of the sap through the cortical vessels. It has been imagined by some physiologists, that buds and roots were convertible into each other, from their having observed, that if a shoot be placed in the ground a root will spring from the lower, and a bud from the upper end of it. But Mr. Knight endeavours to show, that the different effect that takes place at the two extremities depends upon the tendency which the vessels of the alburnum have to send their fluids upwards, while the cortical vessels permit the sap to move in the opposite direction. The author is of opinion that it is the same fluid which produces both the roots and the bulbs.

An opportunity having occurred to Mr. Home of examining the vertebræ of the squalus maximus, he discovered, that when the intervertebral substance was opened, a very considerable quantity of a mucous fluid (not less than three pints) rushed out from a cavity that was composed of a ligamentous and elastic substance. When the fluid was discharged the vertebræ were brought considerably nearer to each other. Mr. Home ingeniously conjectures that this fluid, interposed between the bones of the spine, must serve an important purpose in facilitating the motions of the trunk, by serving as a kind of fixed centre, round which the parts may turn with steadiness and velocity. Something of the same kind of structure was found in other animals that Mr. Home examined; but the quantity of fluid was generally greater in fish than in quadrupeds, which, as the author observes, is probably contrived for producing the rapid motions of the spine which these animals perform in swimming.

The second part of the Philosophical Transactions contains several interesting papers, of which we shall proceed to give a concise abstract.

A large fish, of the kind called by Linnæus squalus maximus, was accidentally caught near Hastings; and Mr. Home being informed of the circumstance, took an opportunity of examining it, and especially paid attention to the peculiarities of its stomach. This organ was found to contain an immense quantity of pebbles, mixed with its natural mucus; its interior surface exhibited two different structures, one occupying the cardiac, and the other the pyloric end, the former having the honey-combed or cellular, and the latter the plicated or rugous appearance. Between the two parts there is in this, as in all the different species of sharks, a globular cavity, with which the pyloric portion communicates by a very small orifice, and there is another orifice, nearly of the same size, between this cavity and the intestine. To the pylorus is attached the valvular intestine, the turns of which are peculiarly firm and compact; it is about four feet long, and terminates in an opening formed by the spiral convolutions of the valve, so as to present very much the appearance of a full blown rose. The author remarks, that the squalus maximus is in many respects similar to the common shark; but that in the structure of its stomach it more resembles the whale, and probably feeds on the same kind of food. It may therefore be considered as forming the intermediate link in the gradation of animals, between the whales and the cartilaginous fishes. As it is a common fish in the northern seas, and contains a large quantity of oil, it might seem, at first view, singular that it has not been more frequently caught by the Greenland fishers; but the circumstance is explained by attending to the nature of its respiratory organs, which, unlike those of the whale, do not render it necessary for it to come to the surface in order to breathe.

Many of our readers may recollect a marvellous account, given in the public papers, of a sea snake, that was said to have been cast on the coast of one of the Orkney Isles. A drawing of it was sent to Sir Joseph Banks, and every precaution seems to have been taken, by some gentlemen who resided near the spot, to obtain from those who saw it as accurate a description as possible of every circumstance attending it. It would appear, however, that it was nothing more than the skeleton of a squalus maximus; but that the lively imaginations of those who saw it, by extending its dorsal fin the whole length of the body, by twisting the lower part of the spine, by adding four additional holders to the two which this animal possesses, and by giving them a little more the appearance of legs and feet, most ingeniously metamorphosed it into a snake.

A very interesting case is related by Sir James Earle, in which a calculus was found in the bladder, larger, we apprehend, than any of which we have an equally well authenticated account. It occurred in the person of a gentleman, who, many years ago, received a severe blow across the loins, which produced a paralysis of the bladder and lower extremities. The bladder, after some time, recovered its action, but the motion of the limbs was ever after extremely imperfect. After a lapse of about twenty years symptoms of calculus were experienced, and the presence of a stone was ascertained; but different circumstances conspired to prevent the performance of the operation, until, after ten years more of great suffering, the extraction was attempted by Mr. Cline. A considerable quantity of fragments was removed, but the great mass of the calculus could not be extracted, and after a few days the patient expired. Upon examination after death the bladder was found entirely filled, and even distended by the stone; it weighed forty-four ounces, and was sixteen inches in length. It seemed to have completely occupied the cavity of the pelvis, and to have projected beyond, and rested upon the os pubis. The pelvis of the kidneys and the ureters were much enlarged, and the latter appear to have been the proper receptacles of the urine.

With respect to the composition of this calculus, the paper is extremely defective; we are indeed told that Dr. Powell analyzed it, and found it to consist of the triple phosphate of ammonia and magnesia, with phosphate of lime, mixed with an unusually large quantity of animal matter. But the experiments are not detailed, and therefore the whole rests upon the Doctor's mere assertion, which, however respectable be the character of the individual, is not the kind of evidence which is admitted in foro chymiæ.

A paper by Dr. Pearson on " expectorated matter," although somewhat tedious, deserves to be attended to. He arranges the varieties of expectorated matter under seven different heads: the three first being merely modifications of what we usually denominate mucus; the fourth puriform matter, secreted without any breach of surface of the bronchial membrane; the fifth consisting of a mixture of the former kinds; and the two last, proper pus, proceeding either from vomicæ, or from simple inflammation of the lungs. At present the author confines his attention to the five first species, and proceeds to describe the physical properties of these fluids. With respect to the three first species, which are mentioned as mere varieties of common mucus, although in general we should acknowledge that the description which is given of their visible appearance and consistence is accurate; yet we confess, that we are very doubtful whether they are so well characterized as to form distinct varieties; and, indeed, Dr. Pearson's own experiments lead him to conclude, that they differ from each other rather in the proportion than in the nature of their ingredients. He has noticed one curious circumstance respecting them, that when they were examined by the microscope, " crowds of spherical particles were seen passing to and fro, in currents not unlike those of the blood, except that they were larger." These globules were not affected by heat, nor by several re-agents that were employed; but they were destroyed by strong sulphuric or nitric acid, by solution in potash, or by completely charring the substance in which they are contained. The puriform fluid, which forms the fourth species, differs little in its general appearance from the common pus of abscesses, although Dr. Pearson supposes that there is some difference as well in its properties as in its origin: we should be disposed to consider it as a mixture of proper pus with the albuminous matter of mucus. It contains a very large quantity of globules " similar to those of the blood." There can be no doubt that a substance of this kind is expectorated in large quantities where there is no breach of surface in the membrane of the lungs.

The author next examined the effect of caloric on these different substances. At the temperature of 150°, the albumen which they contain begins to coagulate; and at 160° the process is completed, a curd separating from the fluid in different proportions. When the substances had been evaporated to dryness they attracted moisture from the atmosphere; but this was not the case with the curd, when it was separated from the fluid, and dried alone. The saline substances were examined, apparently with accuracy, and were found to consist principally of muriate of soda and potash; there were also traces of phosphoric acid, of lime, sulphuric acid, magnesia, iron, and perhaps silex.

The different expectorated substances were next exposed to the action of alcohol, ether, water, and acetous acid. The experiments are very minutely, although, as it appears to us, not always very distinctly detailed; nor does there appear to be any conclusions of such importance deduced from them, as to have rendered it necessary to relate them so much at length. The chief information which we gain is, that, besides the alkaline and earthy salts, the mucus contains potash, which is not perceptible by any of the accustomed tests, and which the author supposes is neutralized by albumen, which, for what reason we know not, he calls an animal oxide. Some of the "conclusions," which form the last section of the paper, have been already alluded to, and we shall now notice the remainder of them. Expectorated mucus seems to consist entirely of albumen and water, impregnated with saline and earthy salts; the proportion of the albumen varies from one tenth to one fifty-fifth of the whole, but it is generally about one twentieth. The different consistence of expectorated matter depends upon the different proportions of this substance; the thicker is the fluid the smaller generally is the quantity of saline impregnation; all the animal fluids contain potash, and not, as has been supposed, soda, united to albumen; proper purulent matter contains much less saline impregnation than expectorated mucus. The author observes, " that expectorated matter belongs to the class of coagulable fluids, and not gelatinizable, or, as commonly stated, mucous fluids." The first position is correct; but certainly, unless we entirely banish the term mucous fluids from the language of physiology, there appears to be no substance to which it can apply so directly as that in question.

Before we leave this paper we must observe, that the author expressly avoids mentioning the experiments of any other chemist who has examined the same substance; and, although this be thought impartial, we cannot entirely approve of it, as, in more instances than one, it leads Dr. Pearson to speak of facts as discovered by himself, which had before been distinctly pointed out by others.

Mr. Brande has obtained some interesting results by the application of the Galvanic influence to the animal fluids. It had been proved by Dr. Bostock, in his experiments on these bodies, that the mucous secretions contain a considerable quantity of albumen; but it was doubtful whether there was any part of them to which the term mucus could properly be applied, because those re-agents, which he had pointed out as the tests for mucus, might be supposed to act on the saline ingredients only. In order to separate these Mr. Brande placed saline, and other similar substances in the Galvanic circle, when he found that a coagulation of albuminous matter took place at the negative wire. Mr. Davy, to whom the fact was communicated, explained it upon the principle, that the negative wire abstracts an alkali from the fluid, and thus permits the albumen to coagulate; and Mr. Brande, following up this hypothesis, attempts to prove, that, in all cases, the coagulation of albumen depends upon the abstraction of alkali. The facts related in this paper are important, and the hypothesis is ingenious; but we confess, that we think it requires farther proof, before it can be generally adopted.

The next paper proceeds from the pen of Mr. Home, and is entitled, " Hints on the Subject of Animal Secretion." The hint, although not acknowledged, seems evidently to have been taken from Mr. Brande's experiments, of which we have just given an account; and Mr. Home argues, from the change which electricity is capable of producing on the animal fluids, that it may be a principal agent in the formation of the different secretions. Besides this, which the author may probably consider as a direct evidence in favour of his opinion, he brings forward some considerations, which show the inadequacy of the common opinion to account for the facts. We must confess, that we did not expect that a writer of Mr. Home's celebrity would have brought forward so crude an opinion, or that the learned body, to whom it was communicated, would have thought it worthy of being inserted among their transactions. Notwithstanding, however, the unfavourable opinion which we express of Mr. Home's paper, we think that the " hints" ought not to be neglected, and we hope that in his, or some other hands, they may lead to some curious and important conclusions.

We are always pleased to meet with the productions of Mr. Knight's pen; for we are certain to obtain valuable information, conveyed in an unassuming style, without any undue affectation of science, or any encroachment upon the discoveries of his fellow-labourers. In a letter, which is inserted in this volume, he makes some curious observations " on the comparative influence of male and female parents on their offspring." His opinion is, that the female character is much more conspicuous than that of the male, and he builds it on the result of some experiments, in which he has lately been engaged, on the production of new varieties of the apple. Mr. Cline had recommended, in the breeding of cattle, that large females should be employed, upon the principle, that the size of the fœtus is chiefly regulated by that of the mother. The principle is probably correct; but Mr. Knight, from his own experience, disapproves of the practice, because, when a large female is made use of, although the limbs of the offspring may be of greater size, yet, with respect to the purposes of utility, an animal is to be preferred which has small limbs, with a proportionably large quantity of muscle. The author supposes, that the female is principally concerned in determining the sex of the offspring, because he has noticed among cattle, that particular females are found to produce a much greater quantity of one sex than other females, although they have both been impregnated by the same male. On the contrary, he has never been able to observe, in the offspring of an individual male, any considerable difference in the number of the two sexes that is produced.

In our last volume we gave a short abstract of a paper of Messrs. Allen and Pepys on respiration, and we have much pleasure in being able to announce a continuation of their labours. They observe, that one of the most remarkable of their former results was the fact, that after the respiration of pure oxygen, a considerable quantity of azote seemed to be discharged from the lungs, more than could be accounted for by the air remaining in them at the commencement of the experiment; and it farther appeared, that this azote was given out in greater proportion in the early part of the process. The experiment was very accurately repeated; 2668 cubic inches of oxygen were respired for 13 minutes, and portions of the gas were removed at different periods for examination. The air was found to be diminished in bulk by 124 cubic inches; and, upon analyzing the different portions, it was found that all of them had gained 10 per cent. of carbonic acid; that all of them had lost oxygen, and acquired azote; but the loss of the first, and the gain of the second of these bodies, were in greater proportion in the earlier than in the later stages of the experiment. The whole quantity of azote produced was 105 cubic inches, and, during the first half of the experiment, its production, in equal times, was nearly in the ratio of the numbers 41½, 20½, 12½, 10, and 7. Here the question occurs, Whether this azote be derived from a quantity of it, which existed in the lungs at the commencement of the experiment, or whether a portion of the oxygen be converted into, or be replaced by, an equal bulk of azote? In order to determine this point, the authors thought it necessary to reconsider the estimate that had been formed of the capacity of the lungs after a complete expiration. It was found, agreeably to our former experience upon the subject, that by no method, but that of cutting the lungs into small pieces, and squeezing them by a powerful press, could all the air be expelled from them. They were then found to be nearly of the same specific gravity with water; and, by an easy calculation, the contents of the lungs, which they examined, appeared to be 1018 cubic inches, a number which remarkably coincides with that proposed by Dr. Goodwyn. The authors remark, after this experiment, "we are thus almost compelled to allow, that, when pure oxygen is respired, a portion of azote is given off by the blood." This conclusion, which appears to be warranted by the present state of the facts, we conceive, affords a pretty strong presumption in favour of that theory of respiration, which supposes that oxygen is actually absorbed by the blood in the lungs. A series of experiments were then entered upon with Guinea pigs; and, first, the animal was confined in a quantity of atmospheric air. The bulk of the air was not affected, nor did the quantity of azote appear to be altered, but part of the oxygen was converted into carbonic acid; the result corresponds with what takes place in the human subject. The animal was afterwards placed in oxygen gas; 1000 cubic inches were employed, they were diminished in quantity by 4 cubic inches, 106 inches of carbonic acid were formed, 54 of oxygen were deficient, and 50 of azote were added. This experiment, therefore, agreed with the corresponding one on the respiration of man, but with this additional circumstance, that the quantity of azote produced is considerably greater than the whole bulk of the animal, which appears to have been no more than 39 cubic inches. The authors observe upon these experiments;

"It seemed that, when the usual proportion of azote was not present in the gas respired, there was a disposition in the blood to give out a certain quantity in exchange for an equal volume of oxygen; and we resolved to try whether this circumstance would occur when hydrogen was substituted for azote; we accordingly made a mixture, containing 22 parts oxygen and 78 hydrogen."

This mixture was employed; the animal did not appear to suffer any uneasiness, although the experiment continued for an hour; but, towards the conclusion, it became drowsy. It was found, upon examination, that all the hydrogen was consumed, and that there was an increase of azote, in the same manner as when pure oxygen is employed. The authors conclude their paper with a summary of the facts that may be deduced from their experiments. This, however, we think it unnecessary to quote, as we have already stated them in the course of our abstract.

In our account of Mr. Davy's experiments on the decomposition of the alkalies we stated his conclusion, that ammonia, as well as potash and soda, is an oxide, and, like them, contains a metallic basis. Dr. Henry, whose skill as an experimentalist is so well known, has, however, been induced to form a contrary opinion: he conceives that, when ammonia is decomposed by electricity no oxygen is procured, and that the small portion, which has been observed in this process, depended upon a little atmospheric air, which insinuated itself into the apparatus. He then instituted a process for ascertaining whether any water is generated by the decomposition of ammonia; but here again it seemed, that by employing the necessary precautions for bringing the gas to a state of perfect dryness, no water was perceptible. Dr. Henry confirms the fact, which had been before mentioned by Berthollet, that, in the decomposition of ammonia by electricity, the bulk of the gas is nearly doubled; and he agrees with Mr. Davy in his estimate of the proportion of hydrogen and azote generated in the process. He then goes on to describe the method which he has lately discovered of analyzing ammonia, by firing it with oxygen; a method which is much more commodious than any that has been hitherto adopted. Ammonia is also capable of entering into combustion with nitrous gas.

The volume concludes with a paper of Mr. Davy's, which is denominated an Appendix to the Bakerian Lecture. In our review of the lecture we mentioned the results that were obtained by subjecting ammonia to the action of potassium, and the difficulty that there was in reconciling them to other well established chemical facts. Mr. Davy has been assiduously labouring to clear up the difficulty, but, as it would seem, without success; so that this point still remains to be further investigated. He has performed some additional experiments on sulphur, and the other combustible substances, which still more confirm his former conclusions respecting their compound nature.

From the same.

An Improvement in the Mode of administering the Vapour Bath, and in the Apparatus connected with it; with Plans of fixed and portable Baths for Hospitals and private Houses, and some practical Suggestions on the Efficacy of Vapour, in application to various Diseases of the Human Frame, and as may be beneficial to the Veterinary Branch of Medicine. The whole Illustrated by eleven Plates. 4to. pp. 22, *exclusive of the Explanation of the Plates.* Booth.

Essay on Warm and Vapour Baths: with Hints for a new Mode of applying Heat and Cold, for the Cure of Disease, and the Preservation of Health. Illustrated by Cases. With an Appendix, containing Observations on Scrophula, and on Pulmonic Complaints, with Cases treated by the Vapour Bath. By Edward Kentish, *M. D. Physician to the Bristol Dispensary. Second Edition.* 8vo. pp. 178. Mawman.

EVERY practitioner, who has paid any attention to the operation of heat and moisture on the animal economy, must have had frequent occasion to lament the difficulty of resorting to it as a remedy, in consequence of the paucity of establishments for warm bathing in this country. A very small proportion, not merely of the population, but even of professional men, have had personal experience of its effects: hence, the general knowledge on the subject is extremely scanty, and the popular notions respecting it are very erroneous. Warm bathing is deemed a "relaxing" and "enervating" practice, calculated only to gratify the effeminate voluptuaries of eastern countries; and adverse to the hardihood of the British character, and incompatible with the mutability and rigour of the British climate—at once weakening the physical strength, and rendering the body more obnoxious to the injuries occasioned by cold.

We are persuaded, however, that these are prejudices which have no other foundation than hypothetical views of the operation of heat and moisture on the living body, chiefly deduced from their effects upon dead matter, or perhaps from the occasional abuse of the warm bath: and we are equally convinced of the great efficacy of warm bathing in relieving many morbid conditions of the human body, as well as of its total freedom from those hazardous consequences which are usually apprehended from its use. We were, therefore, gratified to observe the attention of the profession and the public strongly called to this subject by the two essays before us, and especially by the former; which, being the production of a gentleman of independence, the Honourable Basil Cochrane, can only be attributed to motives of generous and disinterested benevolence, and to a desire of extending to others the benefits which the writer has personally experienced from the practice.

We cannot, however, speak in terms of commendation of the manner in which Dr. Kentish has manufactured his volume; we say *manufactured;* for truly the labour of head which has been exerted in its production is of very small account. The general observations on the subject of warm and vapour baths, are divided into six chapters, and occupy 84 pages: the rest of the volume being made up of the histories of cases, illustrative of the effects of the vapour baths; and of an appendix, containing additional cases, and some remarks on scrofula and pulmonary diseases.

In a short introductory chapter we are informed, that Dr. Kentish formerly possessed an establishment of hot, cold, and vapour baths, at Newcastle, and that he has now completed a similar establishment at Bristol, whither he has removed. In order to convince us that, "in noting the effects of heat, he does not mean to deny or appreciate the value of the effects of cold," he presents us with a brief rhapsody, which he calls reasoning;" *videlicet,*

"Nature herself delights in change—the seasons change, day and night, heat and cold, wet and dry succeed each other; the human system constantly changes; there is an alternate contraction and expansion of the heart; activity and rest, hunger and satiety follow in their turns. This train of *reasoning* might be carried much farther in physical causes, and would even *bear comparison* in elucidating many moral effects; but enough has been said to show, that it would be wrong to have too great a bias towards the application of either heat or cold." p. 3.

Chapter II. which professes to treat of "baths in general," consists of a considerable portion of the article BATHS, which is to be found in Dr. Rees' New Cyclopædia, and in which the construction and different apartments of the ancient baths of Rome are described. With the same liberal hand, Dr. Kentish has dispensed the compositions of Tooke and Savary, in Chapter III. (if, indeed, he has not taken the greater part of it from the same Cyclopædia, Article BATHING, where, at least, the greater part of the same language is to be found) as well as of Clavigero, in describing "the vapour baths of different nations, more particularly as used by the Russians, Turks, and Indians.") As the works of these writers are in every one's hands, it were superfluous in us to repeat the sounds of the echo, or to be the retailers of a twice, nay, of a thrice told tale.

In the fourth chapter the author begins to speak for himself: it relates to the "Functions of the Skin," but chiefly to the "importance of applying heat and cold to the surface of the body in rheumatism and gout." p. 42. Only one of the functions of the skin, however, is here touched upon, namely, the perspiration, and that with a light and rapid hand. The skin, which "is formed by the minute terminations of *all* the superficial and exhalant arteries, nerves, veins, and absorbents of the system," is covered, we are told, "by the colouring, or *blushing* membrane!" Whence it is to be inferred, that the African is black only in consequence of a perpetual *blush*, which suffuses him à capite usque ad calcem! this is a function hitherto unassigned to the *rete mucosum*, the colouring matter of which, and the faculty of blushing, are exactly in the inverse proportion to each other. The principal view which Dr. Kentish takes of the function of perspiration relates to the balance of this and of other functions, which he deems the great constituent of health; and, therefore, the cleanly, soft, and proper secreting condition of the skin is considered as one of the most direct means of warding off disease, and of expelling it when present. The different forms of chronic rheumatism, the author asserts, (and his observation is corroborated by the experience of Dr. Bardsley, of Manchester) have often yielded to the influence of the vapour bath, after they had resisted all the ordinary resources of the practitioner. His remarks on the external treatment of gout, which appear to be hypothetical, and not the result of experiment, lead to no satisfactory conclusions.

Treating of "the effects of heat on the living system," in Chapter vi. Dr. Kentish infers, from a contrasted view of the torpor occasioned by cold, with the increase of sensibility and activity produced by heat, that cold is, in all cases, a sedative, and heat a stimulus to the animal body; which, in fact, almost amounts to an identical proposition. Heat is, obviously, the great stimulus of life throughout all nature, and the abstraction of it is necessarily productive of the opposite effect: nevertheless, as Dr. Currie has well pointed out, the *sudden* abstraction of it, inasmuch as it excites a powerful impression on the sensorium, is stimulant, and rouses the dormant action of the heart, (in syncope, for instance) as quickly as any other stimulus. But this by the way. A doubt cannot be entertained for a moment that the continued application and abstraction of heat increases and diminishes the actions of the living body respectively: and upon this principle, independently of a host of direct experiments in confirmation, it were easy to show the nature of the influence of heat and cold, and the error of the popular views of the subject.

The doctrine of excitement, and of the two species of debility, (which, so far as it relates to the operation of stimulants on the living system, is a correct generalization of facts) is well illustrated by the effects of the stimulus of heat. The temporary abstraction of heat, as in the cold bath, is soon followed by an augmented action of the system; the continued and excessive abstraction of it is succeeded by loss of sensibility, of strength, and, if the continuance or degree of abstraction be sufficient, of life itself. On the other hand, a moderate increase of the stimulus of heat, applied for a short time, like a moderate excitement by wine, occasions an augmentation of the animal powers; and it is only when applied in excess, or continued too long, that indirect debility ensues. Now, it would appear evident, *à priori*, that, in proportion as the temperature of the body had been augmented short of the production of this indirect debility, the application of cold, *i. e.* correctly speaking, the abstraction of a portion of its heat, must be safe; partly because there is then a superabundance present; and partly because, while the strength remains entire, and even greater than usual, the process of evolving heat in the system is carried on in an unusual degree. Hence Dr. Currie concluded, that "before profuse perspiration has *dissipated the heat*, and fatigue *debilitated the living power*," the cold bath is perfectly safe and refreshing; and from a variety of facts he also inferred, that, "in proportion as the body is heated, from whatever cause," (with the provisions just mentioned, and that no local disease has occurred) "in the same proportion is the safety with which cold may be applied." This, however, is not to be understood in an unqualified manner. If the cold be applied until the heat of the body be reduced below the natural standard, and after the heart and arteries have subsided into their usual state of action, the farther abstraction of caloric becomes hazardous. The mischief which arises from exposure to cold after being heated, is occasioned, not by the presence of excessive heat, but from the circumstance of having become *cool*, *after* the excess of heat. The experiments of Sir Charles Blagden, Dr. Fordyce, &c. in a room heated to upwards of 200°, which were repeated by Dr. Dobson and others at Liverpool—the constant practice of the Russians, of passing from a hot bath to roll naked in the snow—the habit of the Romans of pouring cold water over the body, immediately on emerging from the hot bath, &c.—all prove the correctness of the principles above stated. The first mentioned gentlemen " passed out of the heated room, after every experiment, immediately into the cold air, without any precaution;" and after exposing their naked bodies to the great heat, and sweating violently, " we instantly went into a cold room," says Sir C. Blagden, " and staid there even some minutes before we began to dress, yet no one received the least injury."* And Mr. Park assured Dr. Currie, that, in repeating those experiments at Liverpool, " after remaining some time in the stove, where the heat was as high as 202°, he went into the external air without a great coat, or any other than his usual clothing, *during a hard frost*, and perceived neither injury nor inconvenience."†

In combating the general apprehension of " catching cold," after the use of the warm bath, Dr. Kentish justly observes, that if the skin is rubbed dry, and the absurd custom of going to bed, and sweating, be avoided, " we are less liable to partial torpor, or cold in any part, after warm bathing, than at any other time,"—and that, " after thus using a warm bath, *we are the least sensible to the impression of cold.*" p. 84. That this is the fact we know from personal experience; having frequently gone to the warm bath, chilled by the influence of the external atmosphere in the winter, and returned from bathing with a sensation of general warmth, which the cold air was unable to diminish. This is farther confirmed by the testimony of Count Rumford, whom Dr. Kentish has quoted. " So far from feeling *chilly*," says that active philosopher, " or being particularly sensible to cold, on coming out of the bath, I always found myself *less sensible to cold* after bathing than before. I even observed, repeatedly and invariably, that the glow of health and pleasing flow of spirits which resulted from the full and free circulation of the blood, which (warm) bathing had brought on, continued for many hours; and never was followed by any thing like that distressing languor which always succeeds to an artificial increase of circulation and momentary flow of spirits which are produced by stimulating medicines." (Essay on Warm Bathing.)

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* Philosophical Transactions, vol. lxv.

† Currie's Medical Reports on Water, &c. p. 124, 2d edition.

We have dwelt particularly upon this point, with the hope of contributing to the removal of a popular prejudice, which opposes the use of an important agent in the prevention and removal of disease. In conclusion, we may appeal, in corroboration of the principle above stated, to persons of sedentary habits, who possess but a moderate vigour of constitution, whether they have not marked the different effect of external cold upon their frame when they have gone into the air, chilled already by a cold apartment and want of muscular exertion; or, on the other hand, with a general warmth, and an active state of cutaneous circulation. In the former case the chilliness continues for a considerable time, and is with difficulty overcome by active and continued exercise: in the latter, the impression of the external cold is not perceptible, or is soon overcome by the glow which moderate exercise brings forth.

After describing the structure and uses of the ancient baths Dr. Kentish has observed, that the Romans employed the Piscina, or warm swimming bath, " with a view of keeping up their health, strength, and courage." But he adds,

> " The moderns seem, until lately, to have had a very different opinion with regard to warm bathing: it has been generally regarded as relaxing, weakening, and enervating; and this opinion has descended from the physician to the people, so as to be regarded as an axiom. Marteau, Maret, and Macquart, with a host of continental *names* (writers), have looked upon this as a settled point, and have reasoned accordingly. But surely the opinion of the Greeks and Romans, from their extensive and national use of warm or tepid baths, should claim some attention on what they are so capable of forming a judgment *upon*."—" The allegories of the ancients agree perfectly

with their extensive use of the warm bath: the warm springs were dedicated to Apollo; and so far were they from being esteemed debilitating, that some of them were dedicated to Hercules, the god of strength. Suidas, Eustatius, and other ancient writers, make use of the term *balnea Herculea*, as synonimous with warm baths: not one cold bath was dedicated to Hercules; but he is said to have found a natural warm spring, whither he used to retire, with the view of recruiting his exhausted strength, after any great exertion, from whence he arose a giant refreshed. There existed an altar dedicated to Hercules at the baths of Thermopylæ: also in some Sicilian coins this demigod is represented in the act of bathing. Could we forget our prejudices, we should cease to regard warm baths as debilitating: for a great and polished people would not have been so absurd as to have dedicated to the god of strength that which they thought possessed an opposite *effect* (quality)." p. 20.

This may be deemed a corroboration of the fact, that the moderate application of the stimulus of heat increases the strength; but the more direct testimony of the author's experience would have been more satisfactory. The universality of the use of the hot or vapour bath among the hardy inhabitants of Russia is a farther proof of its tonic qualities: and we could mention several instances of the benefit which has been derived from the warm bath in respect to increase of strength, as well as our own personal experience of it. Where it has occasioned debility, the great heat, the inordinate length of time during which the persons have remained in the bath, the frequency of repetition, the encouragement of much sweating after it, or some other abuse of the practice is probably to be considered as the cause of this result. The Romans, under the Emperors, began to use baths of great heat, and some of them are said to have used the bath seven or eight times a day; a practice which, like all other excessive use of stimulation, must necessarily exhaust the powers of the constitution. "From 12 to 20 minutes is, in general," says Mr. Russell, "the time recommended for immersion, at the commencement of a course of warm bathing, with a temperature of water varying from 90 to 100° of Fahrenheit's thermometer, according to the feelings of the patient." Used in this manner, the warm bath will be found to give strength to the infirm; and both the temperature and the period of immersion may be afterwards increased with impunity.

The effects of the vapour bath are, on the whole, the same as those of the warm water bath; but Dr. Kentish thinks that it possesses greater efficacy in those cases where a free perspiration is desirable, as in cases of chronic rheumatism, and other deep-seated pains, since it does not render the patient hot and restless, which the denser medium is apt to do. It induces perspiration at a lower temperature, and more freely than the water bath: "the vapour bath needs not be heated above 96 degrees, to produce a salutary perspiration; whereas a warm water bath seldom produces the discharge at a lower temperature than 100°, and from that it is used up to 112°, in some of the hot springs at Bath."—The vapour bath is likewise represented as a more effectual and agreeable detersive, or cleanser of the skin, and as of the greatest utility in all cutaneous eruptions. Two other advantages over the water bath are also stated; namely, the greater facility with which it may be applied to particular parts of the body, and its application, by inhalation, to the whole internal surface of the lungs. p. 67.

The cases related by Dr. Kentish, in his appendix, are chiefly histories of pulmonic disease, (viz. asthma, incipient phthisis, and chronic catarrh) and of the various forms of scrofula; with some examples of rheumatism and of cutaneous disease. Scrofula is obviously much connected with the cold and humidity of our climate. Mr. Pearson, who superintended the establishment for the African boys, brought over for education by the Sierra Leone Company, states, that they all died of consumption. He says, "it is remarkable that boys, brought from tropical climates, from the age of 8 to 12, almost uniformly become *scrophulous*" (scrofulous Mr. P. would write). "They bear the first winter tolerably well, but droop during the second, and the third generally proves fatal to them." Sometimes the consumption was preceded by swelling and suppuration of the glands, enlargement of the bones, &c. but, in the females particularly, the consumption was often the primary complaint. These fatal effects of the climate rendered it necessary to abandon the plan. Dr. Kentish justly considers the delusive attempts to harden children born in this climate by thin clothing, &c. as analogous to the change in the African children; and is of opinion, that the perfect and healthy evolution of the system is thus prevented, and that the foundation is laid for scrofula, consumption, and other chronic diseases. The vapour bath has been found extremely beneficial under all these circumstances. We shall transcribe the first case in the appendix, in illustration.

" Case of chronic catarrh, from the change of a warm to a colder climate. Master P—, a young gentleman from the Island of Jamaica, aged 11, has been in England three years, for his education. The first winter he was much subject to coughs and colds, which did not leave him until the summer advanced, although he had every medical attention; the second year his complaints were more severe, nor did they leave him so perfectly as in the preceding year. He never seemed perfectly well; his growth was checked; and he carried the appearance of an animal which had been starved with cold, and ill fed. Very early in the third autumn his former complaints were aggravated, his winter cough returned, and the defluction from the chest was very great: to have looked at him you would have taken him for a little old man; his surface was dry and shrivelled, notwithstanding he wore flannel next his skin. He had, during the former winters, gone through the whole series of means which similar complaints yield to in children of a healthier class. In the first inflammatory attack antimonials, with demulcents, occasional emetics, anodynes, and squills were administered, but nothing relieved him except the approach of summer. His expectoration was much tinged with blood; he was hot and feverish after eating, with a quickened circulation; so that there was every appearance of this local distress of the chest being likely to undermine the constitution, by forming hectic fever. It was then only October; the poor little fellow had therefore a melancholy prospect, if he were not to get relief until the following spring, or rather summer. Having myself seen the result of former means to relieve him, I was induced to add the vapour-bath to the other remedies employed: for the first three or four times of its use a great quantity of dry scurfy indurated cuticle was thrown off; by the time he had used it once a day for a week this ceased; and the bath being used of a moderate heat, the natural secretion of the skin was restored. It is astonishing with what rapidity he recovered by these means; his expectoration gradually ceased; his appetite and spirits improved; in short, he seemed to be new created; both his body and mind acquired increased vigour. By continuing its use occasionally he was perfectly recovered. Instead of making him tender, it, on the contrary, by restoring equal action, gave him such vigour as enabled him to bear the winter's cold, and participate in the out-door amusements of his school-fellows, of which his former state of health rendered him incapable." p. 85 et seq.

The practical importance of the subject might justify us in extending this long article still farther: but we trust that we have said sufficient to call the attention of the reader to the remedy in question. We cannot, however, award any praise to Dr. Kentish for the composition of his volume; for, besides the liberal use which he has made of the writings of others, as before stated, there is great incorrectness of phraseology in numerous places. The quotations which we have made, are not free from such blemishes; but other passages still more incorrect might be adduced. In some of the histories, near the conclusion of the book, he has mentioned a "medicated" vapour-bath, as having been employed with success. Now, it may be collected by implication, from what is said in another part of the volume, that the vapour was impregnated with the essential oil of mint, or some aromatic vegetable. It is impossible that Dr. K. can believe that the vapour-bath acquires any additional efficacy by such impregnation; and the indefinite mention of a "medicated" bath savours so much of the *ad captandum* style, in which "more is meant than meets the ear," that, for the author's credit, we could wish it had been omitted.

Of Mr. Cochrane's work we can make no analysis, as the principal part of it would be unintelligible without the assistance of the plates, which represent the structure of the whole of the apparatus and utensils required for the vapour-bath; first, such as may be employed for a hospital, and has actually been constructed in the Middlesex hospital; secondly, such as may be used in a private house; and, lastly, a portable vapour-bath, with its appendages, and the apparatus for applying steam locally to any diseased part of the body. These plates are followed by most minute estimates of every article requisite to complete the whole apparatus under these different circumstances.

The Hon. gentleman was led to the contrivance and use of the vapour-bath in the following way:

"A very protracted residence in India," he says, "had considerably deranged my constitution, and I began to feel the painful consequences. My chest was loaded with phlegm: I laboured under a severe and almost incessant cough; and my voice, which was feeble and interrupted, sometimes failed me. Accident about this time threw in my way 'Mudge's *Inhaler*,' and I made use of it with something of a prophetic assurance that it would lead to salutary results: and my success was equal, at least, to my expectation. This naturally produced reflection on the superior advantages that might be obtained from vapour, upon an extensive scale, and with a more general application." &c.—"I was, of course, the first to try the efficacy of the vapour I had thus learnt to control; and such were the balmy effects it produced on my constitution, that they appeared to me the operation of magic. I expectorated with ease, and very copiously; the stricture on my chest was removed; I breathed freely; my cough left me; and my whole frame acquired new health and vigour." p. 2.

Mr. Cochrane, however, with the utmost liberality, has allowed the use of his bath, and the attendance of his servants to any one, we believe, who has been desirous of obtaining that benefit. And, though disclaiming all medical science, he states, that he has seen "inveterate catarrhs, chronic rheumatisms, contractions of the muscles, and stiff joints," yield to the influence of the vapour. Two cases of gout appeared to be cured by it; and small calculi were passed, without any of the agony which the patient had undergone before the bath was resorted to. Tooth-ache " has been dissipated by it in a few minutes."

We cannot but consider the public as much indebted to Mr. Cochrane's zeal and philanthropy: and this publication is the more valuable, since Dr. Kentish, for reasons best known to himself, has not given us the slightest intimation of the mechanical means employed by him in the administration of the vapour: and as he has also withheld all minute directions as to its use, we shall add to this article, by quoting the observations of Mr. Cochrane upon this head. After stating that the morning has appeared the most proper period of using the bath, since, after repletion, the patients have complained of more heat and languor than at other times; Mr. C. says,

> " In the early stage of the experiment, it was usual to allow the patient to remain in the bath from 15 to 30 minutes; but, better advised by my medical friends, this practice was discontinued, and from 5 to 10 minutes were deemed sufficient, as a profuse perspiration is induced in the course of 3 minutes. Carrying that advice further, the degree of heat was reduced from 130 to 110 or 120 degrees, and communicated, not gradually, as formerly, but by shocks; varying this mode, however, according to circumstances and the particular situation of the patient. The same friends assisted me in rectifying another error. Instead of allowing the patient to continue in bed two or three hours after bathing, closely covered up, and taking warm liquors to encourage perspiration, he remained there only a quarter of an hour, and then lightly covered; and, since reading Dr. Kentish's essay, he has been rubbed dry, and suffered to go almost immediately into the fresh air; unless the weakened state of his frame, from violence of indisposition, has rendered an indulgence necessary; or in cases that required friction, and in those cases my servants performed that operation according to the mode adopted in India, there called " *shampoing*," &c. p. 5.

The more the practice is known, we are persuaded, that this supposed necessary indulgence will be found less adviseable. This trac concluded with a brief relation of twelve cases, in which the vapour was eminently serviceable; and with a letter from Dr. Hart Myers, who himself received great benefit from its use, and who speaks in high terms of commendation of the bath. There is also a certificate of approbation at the beginning of the volume, signed by seventy-six physicians and surgeons, including the most eminent professional men in the metropolis.

From the London Medical Review.

Observations on Fungus Hæmatodes, or Soft Cancer, in several of the most important Organs of the Human Body: containing also a comparative View of the Structure of Fungus Hæmatodes and Cancer. With Cases and Dissections. By James Wardrop, *F. R. S.* Constable & Co. pp. 206.

THAT faculty of the mind which we term discrimination, is at once the most promising and the least improved source of professional information. It opens to the view of an experienced and enterprising observer a field of immeasurable latitude, inviting by the richness of its soil, and not less by the defect of its cultivation. The abuse of this inestimable faculty, it is true, has been the origin of some evil; but this evil has proved in turn not unproductive of good. Some persons have thought the discovery of error the more useful, because the more impressive method of attaining truth; and since it is contrary to all experience to believe that the human mind can arrive at the latter object by a direct and uninterrupted progression, we may as well avail ourselves of the consolation.

The pathology of glandular affections, arising from constitutional causes, is a branch of surgery which loudly calls for the exercise of a sober and delicate discrimination. How frequent these affections are, how important from every cause, how seldom subdued, how often aggravated by the interference of art, we need not inform those of our readers who have ever made the " grand tour" of a hospital. When we consider that the necessity of their scientific investigation is not more urgent now than heretofore, we are ready to throw reproach upon our forefathers; but it will be better timed when we have remedied the grievance, lest it should descend to our posterity. Perhaps, however, it might upon inquiry appear, that we have given some better earnest of our future success than they did. It is something to have dispossessed a large class of anomalous diseases, of which fungus hæmatodes is not the least frequent or formidable, of all claim or title to a relationship with cancer. It is something to have established the peculiarities of the structure of that disease, which has so many varieties and so many analogies, and to have marked these out distinctly for the researches of future pathologists. For this well laid stepping stone we are indebted to the accurate labours of the unpretending and scientific Baillie.

We think that the habits of the rising class of surgeons are such as to encourage sanguine expectations of the usefulness of their labours. They seem to be not only aware of the importance of a strict investigation of diseased structure, but to be possessed of greater advantages for investigating with effect. Anatomy is daily rising in reputation, and, notwithstanding the quibbles and sophisms of some who have never felt its advantages, and would be glad to keep themselves in countenance, *quocunque modo*, it is now more than ever regarded as indispensable to the education of every class of practitioners; and we maintain that the principles of pathology, which are uniform and alike applicable to every organ of the animal body, can never be understood or applied with advantage in the absence of this fundamental knowledge. The silly hypotheses which have been framed to elucidate the structure and phenomena of carcinomatous, and some other diseases, have their origin in an ignorance of anatomical appearances; and the contradictory statements of writers describing the same things; the oversight of important points, and the elaborate enumerations of frivolous ones; the pompous delineation of healthy and natural, for morbid and preternatural appearances, are circumstances of familiar occurrence, entirely ascribable to the same cause. The mysteries of nature have been rendered doubly mysterious by a long prevailing neglect of anatomy; and nothing will give simplicity and pertinency to reasoning and practice but its universal presidiary influence.

But it would be contrary to observation, with a few exceptions, to expect that those who, from years and experience, appear most competent, should exert themselves in unravelling the texture, and discriminating the nicer characters of disease. These are occupations for men unhackneyed in the routine of business, who are warmed with the juvenile ardour for study and for reputation. It is but too evident that, as men grow old, they become empirical, and, for the most part, sink into a total apathy, as far as regards the science of their profession. To whom then, but the class in which the present author is ambitious to be distinguished, are we to look for the illustrations of local disease and their characteristic signs?—we confess ourselves at a loss to know. These gentlemen shall therefore have our assistance and encouragement; we will forgive their occasional prematurity if they have really useful matter to convey; having a due consideration for the toils and perplexities of a search after truth, and a fellow-feeling for the traveller in a road thickly skirted with error; moreover, we will not omit to applaud the magnanimity of venturing their reputation on so hazardous a stake, in devotion to science.

Fungus hæmatodes, we might almost conclude, was not only a new name, but a new disease for a name; for in the records of surgery how very little do we read of tumours having the distinguishing features of this? It emerged from its obscurity in Glasgow, where it passed for a particular mode of inflammation, and seeming to generate there some contagious fomes, was next recognized at Leeds, whence, under the name which it now bears, it soon spread to London, and has since ravaged the hospitals of our metropolis with incredible and unrelenting severity. The whole tribe of fungi have disappeared before the *currus triumphalis hæmatodis.* Alarmed at the progress of this direful invader, we gladly seize the present opportunity of more cooly surveying, and thus, in some degree, disarming him of his terrors.

Under the denomination of fungus hæmatodes Mr. Wardrop comprehends all those morbid appearances which have been described by Burns as spongoid inflammation, by Mr. Hey as fungus hæmatodes, by Mr. Abernethy as medullary sarcoma, and by various authors as soft cancer. Mr. Hey, we believe, was the first who used the term fungus hæmatodes, and, we think, in his account of the disease, are contained cases which, both in their cause and appearances, are specifically distinct. After an attentive perusal of Mr. Hey's cases, and a comparison of them with others which have fallen under our own observation since the subject was brought before the profession, we think we can distinguish two species of disease therein described; and as it is one of these only to which we conceive the author before us wishes to call our attention, we may be allowed to digress a little to point out the difference in the two affections.

Mr. Pott has described a disease as affecting the calf of the leg, which originates in a small hard incompressible tumour, but which, increasing in size, conveys an obscure sense of fluctuation, and contains "a strangely-distempered mass," and some grumous blood. Instances of this anomalous disease are recorded by various authors, and although the circumstance is not mentioned by Mr. Pott, yet from other sources it will be found that the production of a rapidly-increasing fungus is often the consequence of an opening into the tumour. Mr. Pott described it as caused by a disease and rupture of the posterior tibial artery; but subsequent inquiries have proved, that the arterial trunks in the vicinity of the disease continue uninterrupted. Some, indeed, have still attributed it to a disease of the arterial system, and no doubt can exist that the smaller arteries are subject to a disease of their structure similar to what, in larger ones, disposes to aneurism, but which, in these, causes rupture and the production of a tumour like the present. The fibrine of the coagulated blood will be lodged in the sac, and it will be seen how the rupture of the tumour gives vent to this mass, in the form of a rapidly-increasing fungus, precisely analogous to the fungus cerebri of Mr. Abernethy; like it insensible, and bleeding only from its base. Now, it appears to us, that some of Mr. Hey's cases were instances of the above disease, and would refer to his first, second, fifth, and sixth cases, in which the whole history and progress of the complaint tend to confirm our opinion. The case of Campinet is to us peculiarly convincing; the contents of the tumour, the uncommonly rapid growth of the fungus, and the profuse hæmorrhages from its base, forming a strong contrast with those cases presently to be noticed. The remaining cases given by Mr. Hey will be found to differ much from those above referred to, and to bear a strict analogy to the spongoid inflammation of Mr. Burns, and the medullary sarcoma of Mr. Abernethy. The contents are more uniform and less fluid, being a little viscid sanies and a substance, the nature of which is easily conveyed by the term pulpy or soft cancer. The latter is the disease which the author before us notices, and the nature and appearances of which he has undertaken to discriminate. The propriety of giving the term fungus hæmatodes to the latter affection appears to us doubtful, since we believe Mr. Hey and others intended it should designate the former tumour. That Mr. Abernethy understood by it a disease similar to that described by Mr. Pott is evident from the case which he has related, and, in a nosological view, we consider the term medullary sarcoma less exceptionable, as connecting the disease to a genus of which it is evidently a species.

The disease, as it affects the eye-ball, is first noticed; and the great accuracy with which the progress and appearances of it in this organ are detailed, constitutes, in our opinion, the excellence, and certainly the chief originality of the performance.

"The first appearances of the fungus hæmatodes, when it attacks the eye, are observable in the posterior chamber. The pupil becomes dilated and immoveable, and, instead of having its natural deep black colour, it has a dark amber, and in some cases a greenish hue, giving to the eye very much that appearance which is observed in the sound eye of the sheep, the cat, and in many of the lower animals. As the progress of the disease advances, the colour becomes more remarkable; and it is soon discovered to be produced from a solid substance, which is forming at the bottom of the eye, and gradually approaching towards the cornea. The surface of this substance is generally rugged and unequal, and not unlike what may be supposed to arise from a quantity of effused lymph. In some cases, red vessels can be seen running across the opake body; but these are not the vessels which nourish it, but the ramifications of the central artery of the retina lying above it. When the disease advances still further, the form of the eye-ball begins to alter, acquiring an irregular knobbed appearance; at the same time the sclerotic coat loses its natural pearly white colour, and becomes of a dark blue, or livid hue. The tumour, by its continued growth, finally occupies the whole anterior chamber; and in some cases a quantity of purulent matter collects between it and the cornea. At last the cornea ulcerates, and a fungus tumour shoots out from the portion of the diseased substance, contiguous to the ulcerated cornea; and in other cases, the tumour pushes itself through the sclerotic coat. The substance of this fungus is very readily torn; and when a portion of it is separated, or if it be slightly scratched, it bleeds profusely. In other cases the tumour is of a firmer texture, and if, as sometimes happens, instead of coming through the cornea, it bursts through the sclerotic coat, it then pushes before it the conjunctiva, and thus derives a mucous covering. When the tumour becomes very large, portions of the most prominent parts begin to lose their vitality, and separate in sloughs, which have a very fetid and offensive smell, and are accompanied with the discharge of an acrid sanies."

In the majority of cases in which Mr. W. has known the disease to have taken place in the eye-ball, it has occurred in patients of an early age; an observation applied by Desault to the carcinomatous affection of this organ, although it may justly be considered a striking proof of the dissimilarity of fungus hæmatodes to cancer. Numerous cases are adduced to illustrate the various stages and appearances of the disease in the orbit, and although the reader may probably think these sufficiently prolix, yet we are unwilling to discourage that spirit of observation and minute inquiry which they evince.

These cases clearly prove the enlargement of the absorbent and other glands in the vicinity of the disease; notwithstanding, however, these glands are converted into a substance resembling that which composed the primary tumour, Mr. Wardrop has never known a fungus project after the skin which covered them had ulcerated. These cases lead us to lament the inexpediency of extirpating the eye-ball; even when the operation has been done in an early stage, the disease has speedily returned, and dissection has proved its continuation along the track of the optic nerves, to the brain and its membranes. We are somewhat surprised that Mr. Wardrop's good sense did not lead him to be content with those cases which were the result of his own observation, or the more direct communications of his medical friends, without taking together and re-publishing, from sources not always the most respectable, cases which he *supposes* may have been instances of the disease. The subject did not stand in need of such support, and it only proves to us, amidst a thousand instances, how a vivid imagination will press the most incongruous circumstances into the service of its own favourite pursuit.

The existence of fungus hæmatodes in the extremities is probably more frequent than in any other part of the body. The appearances on dissection very nearly resemble those of the disease in the eye-ball. The tumour commonly consists of distinct medullary portions, or lobes, separated from one another by thin membranous intersections, and a distinct kind of sac, or capsule is formed, surrounding the whole tumour. The swelling has a smooth and very elastic feel, conveying a sense of indistinct fluctuation to the fingers. The growth of the tumour causes ulceration of the integuments, and this is generally followed by the protrusion of a fungus. "It is generally of a rounded form; it has an unequal ragged surface, giving the tumour a raspberry, or cauliflower appearance, and clotted blood often adheres to it. It is of a dark red colour, is easily torn, and bleeds on the slightest friction. It has generally a very narrow neck, and, in some cases, spreads out so much as completely to cover the edges of the ulcerated integuments."

We would here observe, that the protrusion of this fungus is by no means a constant effect of the disease, even in its advanced state. A sloughing process not unfrequently commences, and large portions of the tumour are thus discharged. Mr. Abernethy does not mention the formation of fungus in any of the cases of medullary sarcoma which he has recorded, and we consider this, therefore, an additional objection, if the matter be of any consequence, to the employment of the term which Mr. Wardrop has preferred to designate the disease. We have lately seen an instance wherein the apex of the tumour sloughed; a profuse discharge, partly of blood, took place from the sides of the sore, yet no fungus projected.

Many cases of this disease in the extremities are, doubtless, to be found in the works of former writers, but their descriptions and dissections are so extremely vague and inconclusive, as to add nothing to our knowledge of the subject. Some of these cases Mr. Wardrop has referred to, and has given outlines of three drawings in the collection of the Ecole de Medecine, at Paris, of a disease in the shoulder-joint, called by the French "*carcinome sanglante*," which he *supposes* were cases of fungus hæmatodes. In fact, we know not what such representations may not be *supposed* to have been. The experience of Mr. Wardrop combines with that of Mr. Burns, Mr. Hey, and others, in establishing the necessity of the removal of the limb in which this most horrible of diseases has commenced. "Every attempt which was made to cure the disease by removing the tumour alone was unsuccessful, the disease afterwards returning at the place where the tumour was removed, in the neighbouring absorbent glands, or in some other organ of the body."

That disease which Dr. Baillie has described under the term of pulpy testicle, and which is generally known as the soft cancer of that organ, is considered by Mr. Wardrop as an instance of the fungus hæmatodes. If this be the case, we conceive there must yet remain many varieties of the disease undescribed; since these affections of the testicle differ not only with regard to the disease in other organs, but also as to each other, their contents varying from the unorganized creamy fluid often observed, to the more solid and vascular structure of pulpy cancer. Mr. Wardrop has never met with an instance wherein a fungus protruded in this disease of the testicle, and states, that "the irritation and fever created by the extent of the glandular affections which take place, as well as the state of the testicle itself, seem to put an end to the patient's sufferings before such a change as ulceration, and the production of a fungus could take place." The sense of fluctuation and pulpy feel which this disease gives to the testicle renders the diagnosis between it and hydrocele rather intricate. The want of transparency in the tumour, its progress, and the state of the spermatic chord, the comparative weight of it with a hydrocele of equal bulk, are all to be taken into consideration in judging of the nature of the disease. A particular kind of fullness takes place around the spermatic vessels, which we have observed forming a substance resembling the umbilical chord of the fœtus. This state of parts Mr. A. Cooper observed in the dissection of a subject in whom the thoracic duct was obliterated; and hence Mr. Wardrop imagines it may "not be improbable, that the swelling may arise from a diseased state of the absorbent vessels;" but we think it may at least be equally probable, that the diseased state of the absorbents may be the effect of the disease of the testicle. The extension of the disease to the absorbent glands in the pelvis and abdomen, but too frequently frustrate the intention of extirpation.

Fungus hæmatodes has been found in the liver, spleen, kidney, lungs, uterus, ovarium, and mamma, though its appearance in these parts is generally secondary, the patient having previously suffered from it in the eye-ball or extremities. Mr. Wardrop has adduced instances of it in these various organs from his own observations, or the contributions of his medical friends. Many of these are sufficiently conclusive, as establishing the similarity of appearance of the disease in these and other situations. Some of them are very obscure, and the extreme subtilty of the dissections eludes the possibility of collecting the leading features of the disease. Having described the progress and appearances of the disease in the different organs of the body, our author proceeds by a very faithful comparison of the two, to deduce the circumstances on which the analogy and specific difference between cancer and fungus hæmatodes depend. The two diseases agree, indeed, in the slow progress of their formation, in the thin fetid ichor, and occasional hæmorrhage which succeed ulceration, in the fungous appearance which both may assume, in the contamination of the absorbent glands, in their destructive influence on the neighbouring parts, and in the appearance of the disease in many organs at the same time. The differences, however, between fungus hæmatodes and schirrus are very striking.

“ When it appears in the external parts of the body, and has not yet acquired a considerable bulk, instead of being hard and unyielding, it is soft and elastic, and has an equal surface, giving, in most cases, more or less a sense of obscure fluctuation. Its form, when taken out of the body, is determined and accurately circumscribed, having generally a distinct covering of condensed cellular membrane. In place of the hard, fibrous-looking substance, the principal component part of schirrous tumours, the morbid growth in fungus hæmatodes consists of a soft, pulpy matter, which mixes readily with water, and is hardened by acids and boiling water. It has also been compared, by all who have attempted to describe it, to medullary matter in colour and consistence. When the skin, or covering of fungus hæmatodes has been noded by the progress of the disease, instead of the morbid growth being destroyed by ulceration, a fungus arises from it, and the tumour seems only to increase more rapidly in bulk. If the fungus hæmatodes is not interrupted in its progress, both the original tumour, and the fungous mass growing from it, attain a larger size; and the fungus, instead of having a firm texture, like that which sometimes arises from the cancerous ulcer, is a dark red, or purple coloured mass, of an irregular shape, and of a soft texture, is easily torn, and bleeds profusely when slightly injured."

To these remarks we would add, that the contents of the tumour in this disease appear more homogeneous than in schirrus; and that the fungus which protrudes, differs essentially from that which is formed in cancer. In the latter disease, when the fungus has been mascerated a short time it appears to consist of pencilli of fibres which arise in straight lines from the subjacent parts, and are easily separated down to their origin; in the former disease the fungus appears composed of distinct lobes, like portions of kidney, or liver, and on the slightest injury, such as the removal of the dressings, discharge large quantities of a light coloured bloody sanies from their whole surface. The quantity of this discharge is sometimes incredible. From a small fungus only it will reduce, in a very short time, a stout man to the lowest degree of animation. The weakness is such as to lead many to dread the effect of an operation; yet, when this drain from the system is removed, the transition to health is as sudden as the accession of debility. Fungus hæmatodes also affects organs in which true schirrus has not yet been discovered, and occurs at a period of life at which schirrus very rarely commences.

The concluding chapter, which treats of the texture in which fungus hæmatodes originates, we consider the most faulty part of the production, containing a very crude theory respecting the nature of the disease. From having observed that the optic nerve participated in the disease when it exists in the eye-ball, our author imagines, "that the disease consists in a morbid alteration of the nerve itself; and that a growth of medullary matter takes place in this disease, analogous to what is observed in new formations of bony matter, of fat, of skin, or of blood-vessels." Now so little is really known about the organic diseases of nerves, that we could have wished this speculation to have been a little illustrated by the appearance of the nerves in cases of the disease in other organs. In the few cases were disease has been detected in the substance of nerves, the affection has exhibited no resemblance to the fungus hæmatodes; and we can just as easily conceive how the state of the optic nerve was a mere participation in the disease of the other membranes, as that it was the primary seat of the affection. Mr. Abernethy imagined the contents of the tumour to have been merely an inspissated effusion, and the creamy, or putty-like appearance which we have seen in some glands that were secondarily affected, has led us, in such instances, to doubt its organization; although where a fungus has projected, it has always exhibited a high degree of vascularity.

The plates which accompany the work are remarkable more for their neatness than for the impression which they convey to the mind of the objects intended to be represented. The number of these, and the expense of the work, is very unnecessarily increased by an attempt to delineate the disease when removed from the body; the style is altogether inadequate, in our opinion, to the representation of such chaotic appearances.

We have already had occasion to examine the qualifications of the writer of this essay, and have not the slightest disposition to lower our estimate. That the present undertaking is premature will be obvious to every discriminating reader; but it displays an acuteness of observation, and an ingenuity of thought which bid fair for the advancement of science. Prematurity of publication is a fault—but a fault infinitely more venial than the churlish retention of useful knowledge; and we shall, therefore, in taking leave of Mr. Wardrop, only admonish him to beware that the number of his publications exceeds not the importance of the information which he has to communicate.

Some Account of a Journey to the Frozen Sea, and of the Discovery of the Carcase of a Mammoth. *By* M. M. Adams, *of St. Petersburgh.*

I WAS informed at Jakoutsk by M. Popoff, who is at the head of the company of merchants of that town, that they had discovered, upon the shores of the Frozen Sea, near the mouth of the river Lena, an animal of an extraordinary size; the flesh, skin and hair were in good preservation, and it was supposed that the fossile production, known by the name of mammoth horns, must have belonged to some animal of this kind.

Mr. Popoff had, at the same time, the goodness to communicate a drawing and description of this animal. I thought proper to send both to the president of Petersburgh Academy. The intelligence of this interesting discovery determined me to hasten my intended journey to the banks of the Lena, as far as the Frozen Sea, and I was anxious to save these precious remains, which might, perhaps, otherwise be lost. My stay at Jakoutsk, therefore, only lasted a few days. I set out on the 7th of June, 1806, provided with some indispensable letters of recommendation, some of which were addressed to the servants of the government, and others to merchants, from whom I hoped to derive some advantages. On the 16th of June I arrived in the small town of Schigarsk, and, towards the end of the same month, I reached Kumak-Surka. From this place I made an excursion, the express object of which was to discover the mammoth. And I shall now give a sketch of my journey.

The contrary winds, which lasted during the whole summer, retarded my departure from Kumak-Surka. This place was then inhabited by forty or fifty Toungouse families of the Batouline race. Fishing was their ordinary occupation, and the extreme activity of these people filled me with admiration. The women, old men, and even children, laboured with indefatigaable assiduity in laying up provisions for winter. The strongest went a fishing, the less robust were occupied in cleaning and drying the fish. The whole shores were covered with scaffolding, and the cabins so filled with fish that we could scarcely enter them. An innocent gaiety reigned in every countenance, and all exhibited the utmost activity. The fishermen sang while casting their nets, and others were dancing the Charya, which is a dance peculiar to the country. I cannot sufficiently express the emotions of joy which I felt at the sight of these pleasing scenes.

I was convinced, while upon the spot, that the inhabitants of the north enjoy happiness, even in the midst of the frozen regions.

But what astonished me still more, was the picturesque view of the opposite side of the Lena. This river, which is one of the largest in Siberia, majestically rolls its waters through the mountanious chain of Versché-Jansk. It is here, near its mouth, entirely devoid of islands, and much narrower, deeper, and more rapid, than in any place of its course. The mountains here appear in a great variety of forms. They are of a brilliant whiteness, and of a savage and horrid aspect. Sometimes they represent immense columns which rise into the clouds; sometimes they resemble the ruins of ancient forts, and as if they were parts detached from the mutilated remains of grotesque and gigantic figures.

Further off, the horizon is terminated by a chain of high mountains, where eternal snow and ice dart back the rays of the sun.

These landscapes are of exquisite beauty. An expert draughtsman would look in vain for similar views in any other part of Siberia; and I am not astonished that the picturesque situation of Kumak-Surka should become the object of a national song, known solely on the shores of the Frozen Sea.

The course of the winds having at last changed, I thought of pursuing my route, and I had my rain-deer brought across the river. Next day, at day break, I set out, accompanied by a Toungouse chief, Ossip Schoumachoff, and by Bellkoff, a merchant of Schigansk, and attended by my huntsman, three Cossacks, and ten Toungouses.

The Toungouse chief was the first person who discovered the mammoth, and he was proprietor of the territory through which our route lay. Bellkoff, the merchant, had spent nearly his whole life on the shores of the Frozen Sea. His zeal, and the details he procured me, have the strongest claims to my gratitude. I am even indebted to him for the preservation of my life at a moment of imminent danger.

At first I found great difficulty in sitting upon a rain-deer; for, the saddle being attached by a girth of leather only, it was very insecure, and often occasioned me very disagreeable falls. Besides, my position was very inconvenient for want of stirrups, which are never used among the Toungouses.

On our route we traversed high and rugged mountains, valleys which followed the course of small rivulets, and parched and savage plains, where not a shrub was to be seen. After two days travelling, we at last approached the shores of the Frozen Sea. This place is called by the Toungouses Angerdam, or Terra Firma. In order to attain the mammoth, it was necessary to traverse another isthmus, called Bykoffskoy-Mys, or Tumut. This isthmus, which projects into a spacious gulf, is to the right of the mouth of the Lena, and extends, as I was informed, from south-east to north-east for about thirty or thirty-five wersts.* Its name is, probably, derived from two points in the form of horns, which are at the northern extremity of this promontory. The point on the left hand, which the Russians call, by way of eminence, Bykoffskoy-Mys, on account of its great extent, forms three vast gulfs, where we find some settlements of Jakouts. The opposite point, called Maustach, on account of the great quantity of floating wood found upon its shores, is one half smaller; its shore is lower, and this district is completely inhabited. The distance from one point to the other is estimated at four leagues and a half, or forty-five wersts. Small hills form the higher part of the peninsula of Tumut: the remainder is occupied by lakes, and all the low grounds are marshy.

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* Ten wersts are equal to six English geographical miles.

The isthmus we have mentioned is so narrow at some places, that the sea may be seen at both sides. The rain-deer perform a periodical transmigration every year, during which they abandon these places, in order to proceed, by the Frozen Sea, towards Borschaya and Uitjansk, and, for this purpose, they collect in large troops about autumn. In order to hunt these animals with greater prospect of success, the Toungouses have divided the peninsula into cantons, separated by palings. They frighten the deer by loud cries, which they utter all at once, by letting dogs loose at them, and by fans, which they attach to the palings, and which are agitated by the wind. The terrified rain-deer throw themselves into the water, in order to reach some neighbouring island, where they are pursued and killed by the hunters.

On the third day of our journey we pitched our tents a few hundred paces from the mammoth, upon a hillock called Kembisagashaeta, which signifies the stone with the broad side.

Schoumachoff related to me the history of the discovery of the mammoth in nearly the following terms:

" The Toungouses, who are a wandering people, seldom remain long in one place. Those who live in the forests often spend ten years and more in traversing the vast regions among the mountains. During this period they never visit their homes. Each family lives in an insulated state from the rest. The chief takes care of them, and knows no other society. If, after several years absence, two friends meet by chance, they then mutually communicate their adventures; the various success of their hunting, and the quantity of peltry they have acquired. After having spent some days together, and consumed the little provisions they have, they separate cheerfully; charge each other with compliments for their respective friends; and leave it to chance to bring them together again. Such is the way of life of these innocent children of nature. The Toungouses who inhabit the coast differ from the rest, in having more regularly built houses, and in assembling, at certain seasons, for fishing and hunting. In winter they inhabit cabins built close to each other, so as to form small villages.

" It is to one of these annual excursions of the Toungouses that we are indebted for the discovery of the mammoth. Towards the end of August, when the fishing in the Lena is over, Schoumachoff is in the habit of going along with his brothers to the peninsula of Tumut, where they employ themselves in hunting, and where the fresh fish of the sea furnish them with wholesome and agreeable nourishment.

In 1799 he had caused to be built, for his women, some cabins upon the shores of the lake Onroul; and he himself coasted along the sea-shore, for the purpose of searching for some mammoth horns. One day he perceived, *in the midst of a rock of ice*, an unformed block, which did not at all resemble the floating pieces of wood usually found there. In order to examine it more closely, he clambered up the rock and examined this new object all around; but he could not ascertain what it was. *The year following* he discovered, on the same spot, the carcase of a sea cow. [*Trichecus Rosmarus*]. He perceived, at the same time, that the mass he had formerly seen was free from the ice, and by the side of it he remarked two similar pieces, which he afterwards found were the feet of the mammoth. About the close of *the next summer*; the entire flank of the animal and one of the tusks had distinctly come out from under the ice. On his return to the shores of the lake Onroul he communicated this extraordinary discovery to his wife and some of his friends; but their manner of regarding the subject overwhelmed him with grief. The old men related, on this occasion, that they had heard their forefathers say, that a similar monster had formerly shown itself in the same peninsula, and that the whole family of the person who discovered it had become extinct in a very short time. The mammoth, in consequence of this, was unanimously regarded as auguring a future calamity, and the Toungouse chief felt so much inquietude from it, that he fell dangerously ill; but becoming well again, his first ideas suggested to him the profit he might gain by selling the tusks of this animal, which were of an extraordinary size and beauty. He, therefore, gave orders to conceal carefully the place where the mammoth was, and to remove all strangers from it, under various pretexts, charging, at the same time, some trusty dependants not to suffer any part of this treasure to be carried away.

" But the summer, being colder and more windy than usual, kept the mammoth sunk in the ice, which scarcely melted at all that season. At last, *about the end of the fifth year* afterwards, the ardent desires of Schoumachoff were happily accomplished. The ice which enclosed the mammoth being partly melted, the level became sloped, and this enormous mass, pushed forward by its own weight, fell over upon its side on a sand-bank. Of this two Toungouses were witnesses, who accompanied me in my journey. In the month of March, 1804, Schoumachoff came to his mammoth, and, having got his horns cut off, he exchanged them with Baltounoff, the merchant, for merchandize of the value of fifty roubles. On this occasion a drawing of the animal was made, but it was very incorrect. They described it with pointed ears, very small eyes, horse's hoofs, and a bristly mane along the whole of his back; so that the drawing represented something between a pig and an elephant."

Two years afterwards, being the *seventh from the discovery* of the mammoth, a fortunate circumstance occasioned my visit to these distant and desert regions, and I congratulate myself on having it in my power to ascertain and verify a fact, which would otherwise be thought so improbable.

I found the mammoth still upon the same slope, but completely mutilated. The prejudices against it having been dissipated, because the Toungouse chief had recovered his health, the carcase of the mammoth might be approached without any obstacle. The proprietor was content with the profit he had already derived from it, and the Jakouts of the neighbourhood tore off the flesh, with which they fed their dogs. Ferocious animals of the north pole, white bears, gluttons, wolves, and foxes preyed upon it also, and their burrows were seen in the neighbourhood. The skeleton, almost completely unfleshed, was entire, with the exception of one of the fore feet. The spondyle, from the head to the oscoccygis, a shoulder-blade, the pelvis, and the remains of the three extremities were still tightly attached by the nerves of the joints, and by strips of skin on the exterior side of the carcase. The head was covered with a dry skin; one of the ears, well preserved, was furnished with a tuft of bristles. All these parts must necessarily have suffered by a carriage of 11,000 wersts. The eyes, however, are preserved, and we can still distinguish the ball of the left eye. The tip of the under lip has been eaten away, and the upper part being destroyed, exhibited the teeth. The brain was still within the cranium, but it appeared dry.

The parts least damaged are a fore foot and a hind one. They are covered with skin, and have still the sole attached. According to the assertion of the Toungouse chief, the animal had been so large and well fed, that its belly hung down below the knee joints. This mammoth is a male, with a long mane at his neck; but it has no tail and no trunk. The skin, three-fourths of which are in my possession, is of a deep grey, and covered with a reddish hair and black bristles. The humidity of the soil, where the animal has lain so long, has made the bristles lose some of their elasticity. The entire carcase, the bones of which I collected upon the spot, is four archines and a half high, by seven long, from the tip of the nose to the coccyx (about nine feet high, by fourteen feet long) without, however, comprehending the two horns, each of which is a toise and a half long, and both together weigh ten pouds (nearly 400 pounds.) The head alone weighs eleven pouds and a half ($4\frac{1}{2}$ cwt.)

The principal object of my care was to separate the bones, to arrange them, and place them in safety. This was done with the most scrupulous nicety; and I had the satisfaction of finding the other shoulder-blade, which lay in a hole. I afterwards caused the skin to be stripped from the side upon which the animal had lain; it was very well preserved.—This skin was of such an extraordinary weight, that *ten persons*, who were employed to carry it to the sea-side, in order to stretch it on floating wood, *moved it with great difficulty*. After this operation I caused the ground to be dug in various places, in order to see if there were any bones around, but chiefly for the purpose of collecting all the bristles which the white bears might have trodden into the wet ground on devouring the flesh. This operation was attended with difficulty, as we wanted the necessary instruments for digging the ground. I succeeded, however, in procuring, in this manner, more than one poud (forty pounds weight) of bristles. In a few days our labour was ended, and I found myself in possession of a treasure, which amply recompensed me for the fatigues and dangers of the journey, and even for the expenses I had incurred.

The place where I found the mammoth is about sixty paces distant from the shore; and from the fracture of the ice, from which it slid, it is about one hundred paces distant. This fracture occupies the middle, precisely between the two points of the isthmus, and is three wersts long, and even in the place where the mammoth was this rock has a perpendicular elevation of thirty or forty toises. Its substance is a clear ice, but of a nauseous taste: it inclines towards the sea; its summit is covered with a bed of moss and friable earth half an archine in thickness. During the heat of the month of July a part of this crust melts, but the other remains frozen.

Curiosity prompted me to ascend two hillocks equally distant from the sea. They were of the same composition, and also a little covered with moss. At intervals I saw pieces of wood of an enormous size, and of all the species produced in Siberia; and also mammoth horns in great quantities, frozen between the fissures of the rocks. They appeared to be of an astonishing freshness.

It is as curious as it is difficult to explain, how all these things should be found collected here. The inhabitants of the coast call this kind of wood *adamsohina*, and distinguish it from the floating wood, which, descending the great rivers of Siberia, falls into the ocean, and is afterwards heaped upon the shores of the Frozen Sea. This last kind they call *noahsohina.* I have seen, in great thaws, large pieces of earth detach themselves from the hillocks, mix with the water, and form thick and muddy torrents, which roll slowly towards the sea. This earth forms, in different places, lumps which sink in among the ice. The block of ice where the mammoth was found was from thirty-five to forty toises high; and, according to the account of the Toungouses, the animal, when first discovered, was seven toises from the surface of the ice.

The whole shore was, as it were, covered with the most variegated and beautiful plants produced on the shores of the Frozen Sea; but they were only two inches high. Around the carcase we saw a multitude of other plants, such as the *cineraria aquatica*, and some species of *pedicularis*, not yet known in natural history.

While waiting for the boats from Terra Firma, for which I had sent some Cossacks, we exerted all our endeavours to erect a monument to perpetuate the memory of this discovery and of my visit. We raised, according to the custom of these countries, two crosses, with analogous inscriptions. The one was upon the rock of ice, forty paces from the shelf from which this mammoth had slid, and the other was upon the very spot where we found it. Each of these crosses is six French toises high, and constructed in a manner solid enough to brave the severity of many ages. The Toungouses have given to one the name of the cross of the ambassador, and to the other that of the cross of the mammoth. The eminence itself received the name of Selichaëta, or mammoth mountain. This last will, perhaps, some day or other, afford some traveller the means of calculating, with sufficient precision, how much the mountains of ice lose annually of their primitive height.

I found a great quantity of amber upon the shores; but in no piece whatever could I discover the least trace of any marine production.

Our Cossacks not having arrived in time with the boat, I was obliged to return to the continent with my rain-deer, without waiting for them. The vessel, in the mean time, had cast anchor in the bay of Borchaya, three hundred wersts from the isthmus where I was. We arrived without any accident, after a journey of eight days. A week afterwards I had the satisfaction to see the mammoth arrive. Our first care was to separate, by boiling, the nerves and flesh from the bones. The skeleton was then packed, and placed at the bottom of the hold. When we arrived at Jakoutsk I had the good fortune to purchase the tusks of the mammoth; and thence I despatched the whole for St. Petersburgh.

Are the mammoth and elephant animals of the same species? The teeth of the mammoth are harder, heavier, and more twisted in a different direction than the teeth of an elephant. Ivory turners, who have wrought upon these two substances, say, that the mammoth's horn, by its colour and inferior density, differs considerably from ivory. I have seen some of them which formed, in their curvature, three-fourths of a circle. And at Jakoutsk, another of the length of two toises and a half, and which was an archine thick near the root, and weighed seven pouds. It is to be remarked, that the points of the tusks, on the exterior side, are always more or less worn down. This enables the inhabitants of the Frozen Sea to distinguish the right from the left tusk.

The mammoth is covered with a very thick hair over the whole body, and has a long mane upon its neck. The bristles, of the length of two feet, which were found upon the head, the ears, and the neck of the animal must necessarily have belonged either to the mane or to the tail. Schoumachoff maintains that he never saw any trunk belonging to the animal; but it is probable that it was carried off by wild beasts; for it would be inconceivable that the mammoth could eat with so small a snout, and with such enormous tusks, if we do not allow it to have had a trunk. The mammoth, according to these indications, would, consequently, belong to the elephant species. M. Blumenbach, in his system, actually calls it *Elephas primævus.*

The mammoth in my possession is quite different from that found near New-York, which had carnivorous teeth.

Another question still remains to be decided. Has the mammoth originally inhabited the countries of the pole, or those of the tropicks? The thick hair with which this animal is covered seems to indicate that it belonged to the northern regions. To this it does not seem reasonable to start objections, although several writers have done so; but what remains inexplicable is, to ascertain how came the mammoth to be buried in the ice. Two years ago similar relicks were found in the environs of Kirengsk, upon the banks of the Lena, at a greater distance from the sea, and they had fallen into the bed of the river. Others have been found in provinces further south; on the Wolga; and they have been discovered in Germany and in Spain. These are just so many incontestable proofs of a general deluge. It appears undeniable to me, that there has existed a world of a very ancient date; and Cuvier, without intending it, gives evident proofs of this in his system, by the twenty-four species of animals, the races of which are extinct.

Petersburgh, *Aug.* 20, 1807. MICHAEL ADAMS.*

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* The author of the above offers his skeleton for sale, and means to employ the money it shall produce to him, in a journey towards the north pole, and particularly in visiting the island of Ljachow, or Sichow, which, from information received in his late journey, he believes to be a part of the continent of North-America.

Observations on the Structure of the Nerves.

MR. VILLARS, in France, has made many observations on the structure of the nerves, by means of the microscope. He also made corrections in this instrument, in order to render it fitter for the prosecution of his researches. The result of them is, 1st. That every fasciculus of nerves is composed of from one hundred to two thousand elementary fibriles, solid, of a silvery whiteness, and surrounded with a medullary milky fluid. 2nd. That this fluid is the same in the cerebrum, the cerebellum, and the medulla oblongata; that it is formed of round, unequal globules, from the one hundredth to the seven hundredth of a line in diameter, and probably serves to nourish the fibriles of which the fasciculi consist. The nerves, instead of being tubular, he concludes to be solid, and that the pulpy matter is external to the nervous filaments.

Mr. Villars also examined the structure of the pia mater. He found that it enclosed an innumerable number of sanguiferous vessels, and several nervous filaments. (Tableau analytique des travaux de la Societè des Sciences, Agriculture, et Arts du department du Bas Rhin, 1808.)

Experiments on the influence of the eight pair of nerves in respiration, have been made by Mr. Ducrotay de Blainville, of which we shall lay before the reader an abstract.

Willis had ascertained long ago, that if a ligature were made on the nerves of the par vagum of a dog, the animal was sure to die; but the effect on his respiration is not mentioned.

Baglivi repeated the same experiment, sometimes by tying, sometimes by cuting the nerves. The dogs always died of inanition in eight or ten days; in one instance only he mentions that the respiration was injured.

Bichât has remarked, that the action of the lungs continues for a time, more or less considerable, after the division of the eighth pair, and of the great sympathetic. M. Dupuytren having repeated the same experiments upon dogs and horses, thinks that those animals in which the eighth pair of nerves is divided, die from asphyxia.

The author has repeated these experiments upon animals of the mamiferous class, and also upon birds; on fishes he has not attempted them, and has performed but one experiment upon a frog, in the class of reptiles.

1. When the eighth pair of nerves was divided, in rabbits, they constantly died at the end of about seven hours.

2. When one of these nerves only was cut across in the same animal, it did not die. The difficulty of breathing, which it appeared to experience at first, soon went off; but if the other was divided six days afterwards, the animal died at about seven hours afterwards.

3. The section of the same nerves was equally fatal to pigeons and fowls, but not for six or seven days, and then they were in a state of complete emaciation.

4. Many experiments carefully repeated, proved that these rabbits and birds inspired as much air after as they did before the operation.

5. The *chemical* phenomena attending respiration did not appear to be affected in these animals by the above experiments, for the same nitrous gas absorbed equal quantities of air which had been respired by these animals before and after the operation.

6. The author, and Mr. Briton, could perceive no alteration in the usual colour of the blood, whether arterial or venous, in the above mentioned animals.

7. In the above mentioned animals, the number of inspirations was constantly diminished for a certain period, but it appeared, that at each inspiration they endeavoured to inhale a very great volume of air.

8. In these experiments, the power of digestion appeared to be entirely annihilated, as Willis and Baglivi have remarked. Nor would the animal eat after they were performed.

9. Finally, the inspection of the bodies of the animals which had been killed by these experiments, proved the following facts: The lungs contained not more than the usual quantity of blood, nor was that blood more than ordinarily dark; the right side of the heart contained blood, the left being empty, or nearly so; the brain exhibited no signs of congestion or effusion; the stomach or crop was always found filled with food which had been taken before the operation. The crops of the birds were distended with a considerable quantity of white liquid, acessant which reddened the tincture of turnsole. (Noveau Bulletin des Sciences, par la Societè Philomatique, mois d'Octobre, 1808.)

Messrs. Fourcroy and Vauquelin, from later and probably more accurate experiments, conclude that bone* consists

1. Of Magnesia,	.0180
2. Of Iron at its maximum of oxidation,	.0018
3. Manganese, do.	.0014
4. Phosphate of lime mixed with carbonate,	.9788
	10.0000

According to these chemists, the existence of iron and manganese in the bones, is not more singular than that of silex in urinary calculi, as they are contained in the food on which animals subsist, and as there is not an animal, and more particularly vegetable substance, in which they are not found. The green colour which bones so constantly take in consequence of strong calcination, is caused, according to them, by the reaction of the lime from the decomposed carbonate on the phosphate of manganese. In their latest researches, they have also detected traces of alumine in bone. (Annales du Museum d'Histoire Naturelle, viii. cahier annìe 1808.)

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* The bones analysed were those of beeves; it is probable those of other animals do not materially differ.

Observations on the Electricity of the Torpedo. By Dr. G. W. Schilling.

THE shock which those experience who touch a torpedo, so nearly resembles that which is given by a coated jar, that many philosophers have compared the fish to a Leyden phial, and have supposed that its power of stunning its prey upon coming in contact with it, was to be attributed to electricity. Some light may be thrown upon this conjecture by the following experiments:

In July, 1764, M. Schilling put into a vessel of sufficient size a torpedo six inches long, and one inch thick. The shock which it gave upon being touched was so violent as to deprive the part which came in contact with the fish of sensation and power of motion. A very disagreeable feeling remained for two hours in the joints of the fingers and arms with which it had been touched, without any inconvenience being perceived in the other parts of the body. Having removed from a magnet a weight of four ounces, which it had supported, the instrument was brought near the fish, which immediately began to move about with violence, although not touched; upon the magnet being brought still nearer, the torpedo was observed to make great efforts to escape; the magnet was then held in contact with the surface of the water, when the fish exhibited signs of great agitation for an half hour, always, however, approaching the magnet, to which it at length adhered like a piece of iron. The torpedo was detached from the magnet with great caution, by means of a piece of wood, contrary to the wish of the animal, which appeared languid; at a distance, however, it recovered its ordinary vigour. At this time it was touched by one of the spectators without giving a shock. The animal soon re-approached the magnet, as if attracted by it, and, after adhering to it for half an hour, it detached itself spontaneously. At this time the fish was evidently more feeble and languid, though still capable of moving about: in this condition it could be touched with impunity. A magnet placed in the bason with it did not prevent its taking nourishment, but when the magnet was taken out of the water it appeared covered with iron dust, as if it had been dipped in iron filings.

To vary the experiments, the torpedo was placed in another bason, filled with water, in which were also put some sand, bread, worms, and little fish. In consequence of this it became more active, but was still incapable of giving any shock. It might be touched with impunity for several days afterwards, at the end of which time some iron filings were put into the water, which soon restored the animal to its primitive vigour; for, a few days afterwards, upon being touched, it gave a severe shock. The magnet was again brought near it; the torpedo adhered to it as before, but detached itself more quickly, and its power of giving shocks was lost as in the first experiment.

M. Schilling has let slip no opportunity since of remarking the singular effect which a magnet produces upon a torpedo, and he has found that the size of the fish is by no means a matter of indifference as to the success of the experiment, and that the strength of the magnet must be in proportion to that of the animal; for, having endeavoured to attract a very slender torpedo, of a foot and a half in length, with the same magnet which was used in the foregoing experiments, he could not succeed in less than twenty-four hours, and he always found, on the contrary, that the small torpedoes were more obedient to the magnet, more violently agitated, and more easily subduced by that instrument.

[*Bibliothèque Britannique, Mois de Nov.* 1808.]

REVIEW.

The Principles of Midwifery; including the Diseases of Women and Children. By John Burns, *Lecturer on Midwifery, and Member of the Faculty of Physicians and Surgeons, Glasgow. With Notes, by* N. Chapman, *M. D. Honorary Member of the Royal Medical Society, Edinburgh, Member of the American Philosophical Society, and of the College of Physicians, Philadelphia; Lecturer on Midwifery and the Diseases of Women and Children, &c. &c.* 8vo. pp. 524. Fry & Kammerer.

BOOKS are written on every subject of science with two express objects; first, to instruct those who may be entirely ignorant of the subject; and, secondly, to enlighten others who may have advanced considerably in it. Under the first of these heads we must class the book now before us, as its pretensions cannot rank higher. We mean not to convey any disparagement by this avowal, since books may be as necessary to the first class of readers as to the second; we wish merely to give the work its proper standing before we proceed to examine more critically its fitness for this object.

Of the great variety of medical books which daily issue from the press, there must be some which hold a higher rank than others; and were they to be graduated, with a careful attention to their utility, we should find few to rise above mediocrity, and very many fall far below it. Many motives might be assigned for this deluge of books, but we shall content ourselves with stating three principal ones, viz. to get into business by having the writer's name constantly before the public; secondly, to get money; and, thirdly, with a view to benefit mankind, by making known useful discoveries and improvements, the result of extensive practice, and long and attentive observation.

Need we say the latter is the only laudable one? We will not pretend to designate which of the two first has influenced our author, but cannot easily conceive it to be the latter, as he is still, we understand, under thirty years of age; and we know too much of European practice, and the difficulty of getting even a moderate share of it, to allow us to believe the author uncommonly qualified to advance any thing very new or important on his subject. But there can be no doubt whatever that the first and second of those motives actuated the Philadelphia editor, who puts his name in the title-page, with the promise of notes, though his few and puny additions are quite insignificant; and his omissions, in two instances, exceedingly dishonest. Mr. Burns, if he gives nothing new, evinces, at least, a spirit of arrangement, and much judicious erudition. If he has nothing on midwifery, strictly speaking, but what is fuller and better in Baudelocque, he has, in his second part, collected with judgment, from an extensive reading, a great many important illustrations, and he has given the young student a useful abridgment; but Dr. Chapman wants to ride on his shoulders into celebrity, without adding an iota of the least novelty from his own stock. This lazy mode of creeping into notice, and this unfair way of imposing useless editions on the student deserves to be censured, as a selfish artifice and a growing evil.

Transcript after transcript continually drops from the press—and hence the production before us. The most superficial survey of this work will at once convince us that the author had but little reliance on himself. He transcribes from almost every authority that has gone before him, but he does not enrich us by any new observations of his own, nor make any just critique on the errors of his predecessors; he, at most, now and then ventures to affirm his experience coincides with the authority he may have just quoted.

Were we asked if the work of Mr. Burns supplies any desideratum in medicine, we should certainly say, No. Smellie, Johnston, Denman, and Baudelocque are in the hands of almost every body. And from whom is this work derived? Principally from the authors just mentioned. It might, perhaps, be desirable and useful, for a man well qualified for the task, to condense, into one view, the excellences of the respectable practioners just mentioned; but we conceive the best qualified for this undertaking will be a man of long experience; one who could justly appreciate what is truly valuable, from having stood the test of practice; one who could correctly estimate the difficulties of the profession, and the best means of overcoming them. In the choice then of materials no common share of judgment is required, and that judgment must be founded on experience. We need not say, a young man of few years practice, and, consequently, of limited experience, is not the fittest person to compile a work which should be almost exclusively practical. There is room, therefore, notwithstanding Mr. Burns's publication, for a work of results, to contain canons of practice, and that can command general assent, as well by the mature experience, as the talents and erudition of the author.

Having said thus much on the pretensions of our author as a practical writer, we shall proceed to say something of the work itself. The author has given the usual divisions of his subject—he commences with the anatomy of the pelvis; and then gives an account of the child's head, and its progress through the pelvis. In this he has done nothing more than copy from preceding writers; but the perspicuity of Baudelocque, whom he has more immediately imitated in his account of the passage of the head through the pelvis, is in great measure lost by his not having followed him more closely. Few subjects of midwifery merit more attention than this, for on it depends every fundamental principle of the art; too much pains, therefore, cannot be bestowed upon it—and on this head Baudelocque merits particular attention. He has described, with luminous precision, the mechanism of labours, the fruit, no doubt, of long and attentive observation, and deep research.

We do not agree with Mr. Burns, however, when he asserts, "that the turning of the face into the hollow of the sacrum is further assisted by the curve of the vagina, and the action of the lower part of the uterus." On the contrary, we maintain, that "the lower part of the uterus" is very frequently the only obstacle to this conversion, and that the curve of the vagina cannot contribute to this effect is certain, as the head must occupy the lower strait of the pelvis before it can arrive at the vagina; and when arrived there the turn is already made, and this by the inclined plane formed by the sacro-ischiatic ligaments.

The diseases of the organs of generation follow the considerations of the pelvis, and the author has enumerated a great variety, but we do not conceive he has thrown any light on this subject; we consider it as a mere transcript from preceding practical authors. He has given us nothing important from his own experience, nor do we find a single observation that challenges attention, or a single prescription that has not been known before.

To this succeeds the history of menstruation, and "the diseased states of the menstrual action." The remarks just made will also apply to this chapter.

This is followed by the history of "conception," "of the gravid uterus," "sterility," "extra uterine pregnancy," "the signs of pregnancy," "the diseases of pregnant women," in which we find nothing either new or interesting, being almost a close copy, in some instances, from Denman, Manning, &c. This concludes the first book.

The second book commences with the "classification of labour." In this the author has attempted some novelty, but without benefiting his subject.

His remarks on natural labour are judicious and correct, but evidently copies. We do not agree with him in all points in his directions for the management of the mother and child, and we particularly object to his mode of securing the funis. We have more than once seen mischievous consequences result from applying the ligature too near the belly of the child. We, therefore, do not hesitate to say his direction to apply it " very near the navel" is always wrong, and may sometimes be fatal. Nor do we agree with him in the belief, that a second ligature is necessary; and if it be not necessary it should not be applied, as it deprives the patient of a useful mean of facilitating the expulsion of the placenta, by the divided vessels discharging several ounces of blood, and, consequently, diminishing the size of the placenta; the placenta, from being emptied of a portion of its blood, contracts itself, which proves a useful stimulus to the uterus, and thus almost ensures its own expulsion.

Premature labour, preternatural labour, tedious labour, instrumental delivery, impractical labour, complicated labour, are next considered. In treating of these the author has followed his predecessors pretty closely, although it could have been wished he had observed the precision of Johnston and Baudelocque in his account of the management of breast and feet presentations. These terminate the second book.

The third book begins with considerations " on the puerperal state." He directs, in his " treatment after delivery," that " if the woman be not much fatigued, she is to turn slowly on her back, and a bandage is to be applied under her, which is to be spread evenly, and pinned so tightly round the abdomen, as to give a feeling of agreeable support." " Next, the wet sheet is to be pulled from below her." This direction may not, at first sight, appear to merit much attention; but when we reflect that the future comfort and perhaps welfare of the patient may be involved in it, we shall be pardoned for adverting to it. We fully appreciate the value of the bandage just recommended, and would always have one applied, but not exactly in the order stated by our author. It must be a primary consideration to put the patient to bed as dry as possible, and, with this view, we should advise the bandage to be applied after the " wet sheet" is removed, and the patient placed in her bed. If the bandage be employed at the time recommended by Mr. Burns, it would infallibly get wet, and subject the patient both to hazard and inconvenience. And we further differ with Mr. B. when he advises the patient, if "wet with perspiration or discharge," to be allowed to rest some hours before these are removed. We do not hesitate to condemn this mode of practice, as much mischief may arise from allowing them to remain so long in wet clothes, as well as great inconvenience to the patient from disturbing her, and unnecessarily exposing her to fatigue; and the more especially, as experience proves it can be done immediately after delivery, without the least injury to the patient, if it be properly conducted.

Under the head "of the puerperal state," Mr. B. considers a considerable variety of subjects, such as uterine hæmorrhage, inversion of the uterus, after pains, &c. In treating of these, he has pursued the path struck out by Johnston, Smellie, Denman, Baudelocque, &c. We find nothing new or interesting in the change of dress their observations have undergone.

Book the fourth is taken up in treating of the "management and diseases of children," in the whole of which, we do not find a single new observation on the nature of these diseases, nor a solitary new prescription to remove or mitigate their violence, although this part occupies nearly one hundred and fifty pages.

Upon the whole, we are disposed to believe the work before us a bookseller's job, and entitled to no more praise than these undertakings generally merit; the author has not pretended to either much experience or novelty; the whole work bears the marks of much modesty and considerable reading, and is, it must be confessed, respectable for the number of its authorities.

We cannot dismiss this subject, however, without declaring, in our opinion, the author fails in enterprise, in more instances than one. We will bring forward one proof of our assertion, not less remarkable than reprehensible: in page 178 he speaks of uterine hæmorrhage, a disease both frequent and formidable, but in which he does not appear ever to have employed one of the most powerful remedies of the materia medica for its suppression, although recommended by the highly respectable authority of Dr. Rush; we allude to the sugar of lead.

We hoped, on seeing an American edition of this work, by Dr. Chapman, that we should find it enriched by valuable notes, and interesting facts; in this we were completely disappointed; we see nothing new, or much worthy of observation; we find two or three instances of querulous attention to immaterial points; a suppression of two or three paragraphs, where a fellow practitioner's name was used; and no inconsiderable departure from his road, to pay a fulsome and overstrained compliment to a professor of the Materia Medica. In p. 178-9 of the London edition, corresponding to p. 213 of Chapman's edition, Mr. Burns has the following passage, " Dr. Rush has informed me, that the acetate of lead given internally, in doses of two or three grains, has often checked hæmorrhage. Dr. Dewees, lecturer on midwifery in Philadelphia, mentions that he has found it very useful in the form of clyster, throwing up a scruple of the salt dissolved in three ounces of water, after having, if necessary, given a common injection to empty the rectum." Again in p. 246 of the London edition, answering to p. 283 of the Philadelphia editor's, is this passage: " Dr. Rush informs me, that in America, it has been used with great advantage, (blood-letting in a rigidity of the parts); and Dr. Dewees has politely sent me a dissertation on this subject, which contains very good cases of its efficacy when pushed freely." Both these passages, in which the Scotch physician mentions Drs. Rush and Dewees with commendation, are omitted by Dr. Chapman, as if they had not existed in the text.

Such a want of candour, and an act of such mean and selfish jealousy, we can not but severely reprobate; but we do not pretend to be sufficiently acquainted with the medical politics of Philadelphia to account for it; it is obvious enough, however, that Dr. Chapman is an enemy to the fame of Drs. Rush and Dewees without fairness, an adulator of Dr. Barton without delicacy.

The Principles of Surgery. By John Bell, *Surgeon. Abridged by* J. Augustine Smith, *of the Royal College of Surgeons, London, and Professor of Anatomy and Surgery in the College of Physicians and Surgeons in the University of the State of New-York. With Notes and Additions. New-York.* Collins & Perkins. 8vo. pp. 562.

THE profession is so well acquainted with the merits and demerits of Mr. Bell as an author, that it is needless for us now to pass judgment upon him. That his work was unnecessarily voluminous and expensive was, we believe, universally admitted; an abridgment, therefore, was much wanted. This task Mr. Smith has undertaken, and the result is now before the profession. The following extracts from the editor's preface will sufficiently explain his views.

" In the edition of them (Principles of Surgery) now presented to the public, care has been taken to select those parts which were deemed most useful and important. Mr. Bell's descriptions of diseases and operations, which are unrivalled by any modern surgical writer in animation and effect, are retained almost *verbatim*, and where they were thought not sufficiently full and minute, the additions which were deemed requisite will be found in a note distinguished by the letter S, at the foot of the page. Where I have differed in opinion from Mr. Bell on a point of practice, his opinion is stated in the text, and mine in a note, so that the reader can adopt that which he may think preferable.

" Since the publication of the first volume of the Principles of Surgery Dr. Jones has written a most valuable work on hæmorrhage; in the appendix the reader will find an account of his discoveries and observations on that subject; and in the third number of the New-York Medical and Philosophical Journal and Review, a most ingenious defence of Mr. Bell, by Dr. Davidge, of Baltimore.

" Since that period also the celebrated Scarpa has written a work upon Aneurism, in which he differs from Mr. Bell, and indeed all his immediate predecessors, as to the manner in which these tumours are formed. Appendix D will enable the reader to judge of the correctness of Scarpa's doctrine.

"I have deemed it unnecessary to transcribe Mr. Bell's observations intending to prove that the inosculating vessels are sufficient to support a limb when the main artery is tied, as no surgeon is now deterred by the fear of mortification from tying any accessible artery in the human body. Three instances of this have, however, occurred, two are related in Wishart's appendix to Scarpa, and one in the Medical and Physical Journal for April, 1809."

"Mr. Bell gives the diagnostic symptoms and appearances which distinguish fractures of the neck, of the thigh-bone from luxations of the hip-joint, but says nothing of the method of reduction to be employed in this latter case. In the appendix G the reader will find directions for that purpose.

"I have retained all that Mr. Bell says upon the symptoms of the stone, on which he is perhaps rather concise, but I thought there was no occasion for making any further observations upon them, as the distinct perception of the stone, by the sound, can alone warrant the operation of Lithotomy. I have omitted the history of the operation and the various ways of performing it, convinced that the gorget is the best instrument and the only one which most surgeons will use. As to its occasionally passing through the fundus of the bladder, or between that viscus and the rectum, the former occurrence is certainly very rare, and both are to be attributed to the operator rather than to the instrument. And what mischief would not that surgeon do who should attempt to cut for the stone with the knife alone, when he was incapable of passing the gorget into the bladder?"

"It is unnecessary for me to point out all the omissions which I have made. They will be found to consist principally of histories, anecdotes, and operations, and machines now no longer in use."

An English Grammar: comprehending the Principles and Rules of the Language. Illustrated by appropriate Exercises, and a Key to the Exercises. By Lindley Murray. *New-York.* Collins & Perkins. 2 vols. 8vo. pp 668. 1810.

ALTHOUGH this work may not fall strictly within our plan, yet it is one of such indispensable importance to every one who wishes to write the English language with propriety, that we cannot forbear announcing the publication of so valuable a book. The price of it too (a matter of great importance when the cost of books generally is so greatly enhanced) is much lower than the English edition.

INTELLIGENCE.

DOMESTIC.

Report on the Yellow Fever which prevailed at Brooklyn in the summer of 1809. *By Dr.* GILLESPIE.

To JACOB MORTON, *Esq. Secretary of the Board of Health of New-York.*

SIR,

I Received your letter of the 30th December last, in which you request me to forward you such information as I had obtained respecting the late yellow fever at Brooklyn. With this request I now comply to the best of my ability, considering the difficulties I experienced, and the reluctance I have met with in many persons to communicate the knowledge of facts which they possess.

The first case of yellow fever which occurred in Brooklyn last season was that of Nathaniel Muller, respecting whom, from the time of his arriving in the town till his death, I give all the information I was able to obtain; namely, the testimony of two persons residing in the same house with him, which is contained in the following certificate.

I hereby certify, that I have lived as a boarder with Mrs. Smith upwards of two years, and that, on the morning of Friday, the 30th of June last, Nathaniel Muller was brought from the ship Concordia, by Capt. Coffin, the master of that vessel, to the boarding-house of Mrs. Smith, he (Muller) being at the time very ill of a violent fever, of which he died between two and three o'clock of the next day, and was buried two hours afterwards, only one person attending the cart. His fever appeared of a most violent kind, and I remonstrated with Mrs. Smith for receiving in her house a person in his bad condition; and further, that Mr. Wilson, (another lodger) and myself were very much afraid of the nature of Muller's disease, and refused, after having seeing him once for a short time, to go again into his apartment, as we were urged by Mrs. Smith, who said she believed him dying about day-light of Saturday, the day on which he died; but we thought his disease of a catching nature, and, with the rest of the family, did not choose to be near his person, nor did any one sleep or sit up in his room, and very few had communication with him at all, and no person for any length of time while he was at Mrs. Smith's house.

Muller told me, during the short time I saw him, on my inquiring what was his disease, that "*he knew he should die, for he believed he had the fever, which he brought from the Havanna.*"

GILBERT BARKER.

Dated Brooklyn, Nov. 26th, 1809.

I do hereby certify, that the above statement, as far as respects myself, to be correct and true.

her
MARY ⋈ SMITH.
mark.

The above Gilbert Parker, and Mary Smith, signed the above in the presence of ROSEWELL GRAVES,
Agent of the Board of Health.

Muller, I have ascertained, sailed from the Havanna in the ship Concordia, Captain Coffin, about the beginning of June last, at which place, I am credibly informed by three respectable persons now in this city, the yellow fever raged all last winter and spring, until the time when they left it, which was on the 23d of May, only a few days before the Concordia. This vessel arrived at the quarantine on, I believe, the 17th of June, and, having complied with the regulations enjoined upon her there, she was permitted to come up, and anchored in the East River; after which she hauled in Sands' lower dock, at Brooklyn, about half way between the Old and New Ferries. On the 29th of June Muller had been discharged and gone to New-York a day or two before, where he sickened, and then returned to the ship at Brooklyn, and from thence was carried in her boat to Mrs. Smith's, where he died the next day.

Immediately after this the officer of the Concordia ordered her hold to be washed and purified with vinegar, which induces, at least, the suspicion, that he apprehended something malignant in the nature of Muller's disease.

The Concordia went down, in consequence of the order of your board, on the 26th July. But the disease, by the 12th of July, had already appeared in different persons. Doctors Ball and Wendall have certified in the public papers, that they had attended five cases of it at that date (see their certificate of August 21st). They also state, in answer to Dr. R. Seaman, that " of those of our patients *first* taken (and we have observed the first case) eight can be traced to persons having connection with different ships."

Mrs. Spencer, who died on the 10th of July, and was the first case of the inhabitants that excited particular alarm in the town, was a sailor's washer-woman; and, moreover, she died in one of the dwelling-houses nearest the Concordia. It will appear too from the map* which accompanies this report, that the disease was, from beginning to end, *almost exclusively confined within a circle of two hundred yards semi-diameter from that vessel.* It is also true, that the early cases and deaths were in Fisher's Row, or Dock-street. Several cases afterwards occurred at the New Ferry, viz. John Jackson, who was sent to the New-York Hospital on the 18th of July, and died on the 20th; and John Donlevy, sent to the Alms-House of this city on the 18th of July, and died the same day. They were both ferrymen, in the employ of Mr. Ammerman, who also died on the 28th July. The following account of the disease in Dock-street, which, as Dr. Ball informs me, was the place alluded to in his certificate, is from a letter published by Drs. Ball and Wendall, July 26th. "That a fever, attended with the most malignant symptoms, which seized, as it were, a whole neighbourhood in a moment, and which, with the rapidity of a pestilence, hurried its unhappy victims from a state of perfect health to their graves in the short period of four and six days, notwithstanding the most prompt and assiduous medical attendance, is a fact too awfully true to be in the least questioned."

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* This map is in the possession of the Board of Health.

Two different causes have been assigned by the public, as well as by physicians, for this epidemic. The one, that it derived its origin solely from local causes; the other, that it was to be attributed to a contagion imported at a season of the year when the local circumstances of the place were favourable to its extension. It is stated, in favour of the first, that it originated from an extraordinary assemblage of nuisances in the lower parts of the town of Brooklyn, consisting of a variety of putrefying vegetable and animal substances, together with water lodging in many low, sunken places, particularly at the New Ferry, and also that it was partly owing to the contents of the *mud turtle** being exposed for some days on the wharf of that ferry, so as to be offensive to the neighbourhood.

But to this it might be answered, that as the part of the village here spoken of has, for several years past, been in nearly the same situation as respects cleanliness as it was last season, and as many of the summers of those years were extremely sultry and favourable to the production of yellow fever; nay, when it has actually raged in New-York, and once at the Wallabout,† and not at this spot, it is most extraordinary that it should have made its first appearance here, and destroyed from thirty to forty lives in a summer so very remarkably cool as the last. As to the fact respecting the relative accumulation of nuisances last season and those that preceded it, I am not competent to judge. On this point, therefore, I shall content myself by subjoining the two following letters.

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* A machine for cleansing docks.

† A place about on mile distant, north of Brooklyn, on the same shore.

New-York, December 12th, 1809.

SIR,

Agreeably to your request, I took particular notice in surveying the sickly neighbourhood at Brooklyn, concerning the elevation of the ground above high-water mark, as well as the situation of the streets, yards, or buildings; and found the same as follows, to wit:

To give an opinion in general of the ground and situation of that part of Brooklyn, I find the ground along Front-street from twelve to twenty feet above high-water mark, and descending along Dock-street and Main-street to Water-street, over a sandy soil. Water-street and that part towards the river is new filled ground, and only three feet above high-water, but it does not appear to me to be more filthy than is common in streets adjoining wharfs and slips in this city. The only places appearing to me to be offensive are behind the gin distillery, and a narrow street leading from Dock-street to the Old Ferry, occupied by stables, &c. &c.

The yards behind Brooklyn Hotel and Capt. Smith's are also occupied by stables, where manure is collected; but I could not perceive any offensive smell, and, in general, the yards are roomy and clean. Back-Alley is a high, open, and clean street, having only seven dwellings and two work-shops on it. The store-houses erected along the wharfs in that part of Brooklyn are clean, and mostly new buildings.

I am, with great esteem, your obedient servant,

CHARLES LOSS,

Dr. Gillespie. City Surveyor.

New-York, Dec. 21st, 1809.

DEAR SIR,

Agreeably to the request of yourself, and the Secretary of the Board of Health, I went to Brooklyn yesterday, in order to obtain information from the inhabitants there respecting the origin of the late epidemic which prevailed in that place.

The first inquiry I made was of a Mr. Chichester, who resides near the New Ferry. He informed me that the first case was a man by the name of Muller, who came in the ship Concordia from the Havanna, and that he died at a Mrs. Smith's about the first of July—that shortly after *the fever* made its appearance at some boarding houses, at the lower end of Fisher-street, and soon spread to different parts of the village, particularly near the New Ferry; he thinks the village was as cleanly as in former years.

The next person I conversed with was Henry Stanton, who stated the origin and progress of the fever much as Mr. Chichester. On inquiring of him the relative state of the village (as it respected nuisances) compared with former years, he replied, that, excepting a quantity of pickled herrings, which were stored in Sands' white store, and the sheds near it, the mud taken from the slip by the mud machine, and which lay on the dock and at the ferry-house and yard belonging to Ammerman the ferry-master, the village was as cleanly as it had been for twenty-eight years, the time he had resided there.

From thence I proceeded to the houses mentioned by Chichester in Fisher-street, where I saw Mrs. Magee, at whose house four persons sickened, (two of whom died) about the middle of July. Saw Michael Harvey, who lives opposite Magee's, and at whose house four persons died, from the 10th July to the 6th of August. He did not appear to recollect dates, but thinks a Mrs. Spencer died on the 10th July; a Mr. Bennet, who lived next door to Harvey, died about the 20th July, and a Mr. M'Intire about the same time. I then proceeded to Mrs. Smith's, at whose house Muller died, and obtained the certificate of his case, signed by herself and Gilbert Barker, a boarder with her. I then waited on Dr. Wendall, and requested of him the dates of the deaths of those persons who died in Fisher-street, as they were his patients, but could not obtain any information from him.

I then called on Mr. John Doughty, the town clerk, from whom I got a list of the deaths of all the persons that died in the village during the last summer. I also inquired of several other persons, but received but little information from them: with the most of them there appeared a studious reserve.

I am, Sir,
Your most obedient
Humble servant,
R. GRAVES.

Dr. Gillespie.

I will only add, that as to the mud-turtle, she commenced cleansing the New Ferry slip July 15th, more than a fortnight after Muller's death: and besides, the mud, which consisted of innocent bluish clay, was removed on the afternoon of the 20th. My informant, Mr. Chichester, who resides near the spot, says he stood for some time on this accused baked heap of clay, some of which he crumbled with his hand, and applied to his nose, but could not even discover any unpleasant smell from it: and that, on Sunday the 16th, the day after it was deposited, the weather being remarkably fine, a great number of passengers of every description were arriving at this spot from New-York all day long. Besides, the machine has been there every season for six or seven years past, particularly three years ago, when she cleansed out all the soft mud of the slip. These circumstances, I think, render it conclusive, that the machine had no agency in originating the disease.

It may, however, be said, 1st. If Muller died of a disease so highly contagious, how did it happen that no person received the disease from him? This is answered by the certificates of Mrs. Smith and Barker.

2d. As twelve passengers arrived in the Concordia, why were not at least some of them affected with fever? This seems a plausible objection at first sight, but, on referring to their bond in the Mayor's office, I find that they consisted of eight Frenchmen from the West-Indies, and four natives of New-Orleans. This class of people, from acquiring a West-India constitution, or seasoning as it is called, together with temperate and cleanly modes of life, have always been considered not very liable to the yellow fever. There were also among them four transient persons, bonded as passengers, all sailors. In fact, the whole of them may be considered as transient; for it is not known whether they continued in this city, or whether they left it immediately.

3d. Why did not the ship communicate the disease to persons that were on board her after she went to Brooklyn? As the vessel was unloaded and laid up, it is not probable that many persons were on board of her, although some doubtless were; but it does not appear that any of them went down into her hold, or remained any considerable time in the cabin, or eat, or slept on board. But within a few days after coming to the wharf her hold was entirely white-washed. Besides, it is never pretended, that a vessel coming to a healthy spot will, at her first arrival, and before the atmosphere is rendered impure, communicate contagion to all that approach her.

I have thus stated such facts as I possess respecting the yellow fever at Brooklyn. These I have endeavoured to obtain from the best information I could on the spot; and I trust it will be found that I have stated them with the candour and impartiality that is due to myself, as well as the respect I owe the guardians of the public health.

I have to apologize for the unexpected delay of this report, which has arisen from the following circumstance: I was incidentally informed by Dr. Wendall, in one of my visits to Brooklyn on the subject of this inquiry, that after the death of Muller, he had addressed a letter to Capt. Coffin, (captain of the Concordia) respecting the condition of that vessel and crew. The reply to this letter, he said, unfortunately contained an injunction of secrecy as to part of its contents. Dr. W. however, assured me he would endeavour to procure the consent of Capt. Coffin, on his return from sea, to my perusing the letter; and in the mean time earnestly requested that I would in no manner till then allude to it. Capt. Coffin did not, as we expected, return here, but, as I have been lately informed by his consignee, has sailed from a southern port on another and long voyage. I hope, therefore, that the Board will excuse a delay in this communication, which has arisen from a desire to connect with it every circumstance I could collect relative to the subject of its contents.

JOHN D. GILLESPIE.

New-York, June 5th, 1810.

Agreeably to our promise, we have inserted Dr. Gillespie's Report to the Board of Health on the subject of the Malignant Fever last summer in Brooklyn. From Dr. Rodgers' Report, inserted in our last number, this letter from Dr. G. and the following observations of Dr. R. our readers will be able to take a view of all *the facts, and decide for themselves upon the origin of the disease in this particular instance. Is it not extraordinary that it should be contended that the disease was imported into Brooklyn from Havanna, when it could not be imported, so as to spread, from Brooklyn into New-York?*

Editors.

To the Editors of the New-York Medical and Philosophical Journal and Review.

GENTLEMEN,

Being informed that you will insert in your Journal of this month the report of Dr. Gillespie on the yellow fever which prevailed at Brooklyn in the summer of 1809, and published in the Medical Register, I have taken the liberty to make a few remarks on the variance of facts, as stated in that report and the one made by me to the Board of Health last autumn—and to confirm what I had before advanced.

Dr. Gillespie says, that the Concordia sailed from Havanna about the beginning of June, " at which place," he is " credibly informed by three respectable persons now in this city, the yellow fever raged all last winter and spring," &c. To this I reply, that the Concordia left the Havanna the 28th of May; and, without impeaching the credit or veracity of Dr. G.'s informants, declare, that, from the affidavits of ten masters of vessels, sailing at different times from Havanna, it appears that the Havanna was perfectly healthy the whole of the months of March, April, and May. I have even one affidavit of Havanna being healthy on the 2d of June, and of its being so on the 12th, though it was then sickly among the shipping.

This intelligence of the shipping being sickly made us interdict the communication between Havanna and New-York before it was sickly ashore. If Dr. G.'s informants are right, then have ten masters of vessels deliberately perjured themselves; but we are not to suppose this latter circumstance. And yet Dr. G.'s informants still have related what they heard, though their information might have been incorrect. It is from the oaths of masters of vessels that I have said, and do believe, that the Havanna was healthy when the Concordia sailed.

In order to inculpate shipping, Dr. Gillespie says that, of the patients first taken, "*eight* can be traced to persons having connection with different ships." This is the truth, but not all the truth: these persons had worked on board of ships, *but not of infected ships*—the ships they had worked on board of were the *Alligator*, the *Brutus*, and the *Phœbe*, neither of which was sickly, or had come from any foreign port, or had impure materials on board. *Not one of them had been on board of the Concordia, or had any connection whatever with any one who had.*

Dr. G. goes on to state, that Mrs. Spencer died on the 10th of July, and was a sailor's washerwoman, "and died in one of the dwelling-houses nearest the Concordia." No particular use is made of this observation, nor any direct inference drawn from it: the reader may, if he please, suppose that Mrs. Spencer washed some clothes of the Concordia, or had been on board—neither was the fact—she never had any connection with the ship. Mrs. Spencer was the wife of a man belonging to the navy-yard, and washed clothes for the seamen belonging to the Wallabout. On her death-bed she was asked if she had been on board of any ship? She replied, that she had been on board the Brutus, to inquire for a man for whom she was bound, but no other, and had only stepped on to the deck and directly back again. Now, the Brutus did not lay near the Concordia, and was healthy; nor did the proximity of Mrs. Spencer's house to the Concordia do her any harm. There were houses and shops, constantly worked in, much nearer the Concordia than Mrs. Spencer's.

It deserves to be remembered, that the people living in the houses and shops nearest to, and *immediately facing the Concordia*, and having most communication, *nay*, *the only people of the village who had communication with this ship*, were wholly and entirely free from disease:

The last point of variance of fact between Dr. G. and myself is respecting the persons who had communication with the Concordia. In the 8th page of his letter, and last paragraph, he says, that " it does not appear that any of them went down into her hold, or remained any considerable time in the cabin, or eat or slept on board."

It has been shown in my report, that the *mate* and *three* seamen lived constantly on board, eat and slept there, and continued well. It appears also, that at least *twelve*, if not more, masters and mates of vessels were in the habit of going very frequently on board, and that they often staid till late at night, so as to make long visits, and stay a considerable time aboard. These were all in the cabin, and the strong probability is, that they eat and drank very often on board: these all continued well. Let it not be said that they took infection to the village of Brooklyn—they did not live in Brooklyn, but went directly to their houses in New-York, and their families all continued well. It appears that *two* men cleaned out the limbers of the ship, and if there had been any filth in the ship would have suffered first: they have continued well. The wharfinger and his son were often on board, in the cabin. It appears then, that *twenty* persons at least were very frequently on board at Brooklyn, and not one has suffered. More than *seventy* persons had communication with the Concordia from the time of her arrival at New-York till her leaving Brooklyn, and have continued well. Although no one pretends that any injury arose from Nathaniel Mullen, yet a certificate is given respecting him, to prove the importation of his disease. Mr. Barker states, that Mullen told him, during the short time he saw him, " that he (Mullen) had the fever which he brought from Havanna." Nothing is said of Mullen previous to this. We know, however, that Mullen was in perfect health the whole of the voyage, and till the 29th of July. He left the ship directly on coming to New-York, and, after a week of carousing, and spent in the usual apartments of sailor lodging-houses, he returned to the ship on the 28th of July, *excessively drunk*. He was in that situation all that day, and lay on the open deck, exposed to the heat of the sun; the night after, still intoxicated, he lay on deck, exposed to the air and dew. In the morning of the 29th he awoke with a violent and raging fever. From such violent exciting causes we, of necessity, expect violent effects. On the 30th, without any proper attempts, that we know, to help him, he was taken to Mrs. Smith's, at Brooklyn, and there he died early on the morning of the 31st. With this state of facts before our eyes, and with such a knowledge of the predisposition of the patient, and of the exciting cause of his disease as we now have, we must conclude, that it would be next to a miracle if he had possessed his reasoning powers when he spoke to Mr. Barker. It appears, by the certificate of this person, that he was under great apprehension of danger, and refused to go a second time into Mullen's room, which he had been in but for a short time. It is very presumable that his state of mind and shortness of visit made him misapprehend what Mullen said to him. It deserves notice too, that nothing of this kind was said either to the physician or nurse, who were with him constantly.

But Mullen could not have brought the disease from Havanna in his own person: he had been thirty days and more from the Havanna, and during that time been perfectly well. Now, it is allowed that febrile contagion will not rest on a clean and healthy subject so long a time without showing itself—the theory of our health laws, and the practice founded on them, is in conformity with this opinion.

Upon the whole, it appears, that no evil whatever arose from Mullen, and that no charge against the Concordia is substantiated; for she had no foul or impure materials on board—no person who had ever communication with her, either at New-York or Brooklyn, was in the slightest degree indisposed; nor had any of those who died at Brooklyn any connection with any person who visited her.

I have abstained from any other remarks on Dr. G.'s letter than what were immediately connected with my report. I refer the candid reader to the report of the Brooklyn Board of Health of last summer, the letter of Dr. E. F. Smith, and the report made to the Board of Health last autumn.

With respect,
I am, gentlemen,
Yours, &c.

August 22, 1810. JOHN R. B. RODGERS.

New Periodical Publication.

The first number of a new periodical work, to be continued quarterly, was published early in July, by Mr. Ezra Sargeant, of this place. The principal object of the editors, who are anonymous, is, we believe, to advocate the foreign origin of yellow fever. Periodical works, however numerous, can scarcely be productive of injurious consequences to the public, and are almost necessarily beneficial, at least in a degree, as they excite discussion, and, by multiplying the number of writers, are the means of saving *facts* which might otherwise be lost.

Prospectus of a new Mineralogical Work.

Mr. S. Godon has issued the prospectus of "A Treatise on Mineralogy, adapted to the present state of Science; including important Applications to the Arts and Manufacures." To be published by Birch and Small, Philadelphia. It will consist of two volumes in 8vo. with one volume of plates in 4to. The work will be put to press as soon as a sufficient number of subscriptions shall be obtained to warrant the publication.

University of the State of New-York. College of Physicians and Surgeons.

The lectures in this institution will commence, as usual, on the first Monday in November. Since the last winter Dr. M'Neven has been appointed joint professor of chemistry, and will deliver the lectures on that subject. As this will necessarily occupy a great portion of his time, he has engaged Dr. James Cocke, late joint professor of anatomy, surgery and physiology in the Medical College of Maryland, to assist him in the discharge of his duties as professor of obstetricks, which chair he has filled for the last two years. Dr. Cocke will be ready to commence his course at the same time. The standing of this gentleman in the profession promises to render him a valuable accession to the talents to be already found in the College of New-York. In addition to the public courses of instruction there will be a private course of lectures on surgery, by the professor of anatomy and surgery, who will also give demonstrations, as opportunities may occur, but to which those pupils only who attend the surgical lectures will have a right of admission.

FOREIGN.

Report made to the Physical and Mathematical Class of the French Institute on a Burning Mirror presented to the Class, by Mr. Peyrard.*

Mr. Peyrard, who has just published an elegant translation of the works of Archimedes, was naturally led to reflect on the means which that great geometrician is said to have employed to burn the fleet of Marcellus before Syracuse. Both the ancients and the authors of the middle age relate, that he used a burning mirror; but none of them enter into the particulars sufficiently to give us an accurate idea of his process. Anthemius, who built the church of Saint Sophia, at Constantinople, in the sixteenth century, and appears to have been a very intelligent architect, invented an assemblage of plane mirrors, to produce the same effect as that of Archimedes. Since that time Kircher, who, perhaps, was unacquainted with the works of Archimedes, thought of something similar. Lastly, Count de Buffon constructed a burning mirror, composed of a hundred and sixty-eight plane glasses; and the experiments in which he employed it are well known. These three processes, which come to the same thing, are attended with serious inconveniences.

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* Sonnini's Bibliotheque Physico-économique, Nov. 1807, p. 349.

For a mirror to reflect to one and the same point, the rays of the sun, considered as parallel to each other, the reflecting surface must make part of that of a paraboloid of revolution, the axis of which is parallel to the rays of light, and its focus their point of union. If this mirror were composed of a great number of plane mirrors of moderate size, the plane of each must be parallel to a tangent of the paraboloid at the point where it is cut by the corresponding radius vector. Now, in consequence of the motion of the sun, the position of the axis of the paraboloid changes with some rapidity. If the form of the mirror, therefore, be unchangeable, the whole must turn round the focus with the sun, which appears to be impracticable: and if the parts that compose it be moveable, independent of each other, each of these parts must turn, so as to be constantly perpendicular to the right line that bisects the angle formed by the solar ray and the corresponding radius vector.

It appears difficult to give the component mirrors the movement in question by means of a machine, less, perhaps, because the change in the sun's declination would render this machine complex, than because the expansion of the metallic rods, used for imparting the motion, would change in a perceptible and unforeseen manner the direction of the component mirrors; and because the action of the machinery would impart to each mirror a vibratory motion, that would keep the image in perpetual agitation.

There remains no other reasonable way, therefore, of composing a burning mirror of several plane mirrors, but by entrusting each of the latter to an individual, charged with keeping it in the proper position for reflecting the image of the sun to a determinate point, varying the position agreeably to the motion of the sun. But Mr. Peyrard justly observes, that this method is attended with an inconvenience, which must prevent its success. It is easy, indeed, for a single person, attentive and conveniently placed, to direct to a point the image of the sun reflected from a mirror of moderate size, and to keep it there, notwithstanding the motion of the luminary. The difficulty would not be very great for three or four persons to do this at the same time. But if fifty, a hundred, or two hundred persons were employed to form a burning focus in this manner, as none of them could distinguish the image he sent from that sent by another, if one of the images alone should deviate from the focus, each of the co-operators would try whether it were his, and hence would arise an agitation and disorder that would prevent the focus from being formed. This inconvenience Mr. Peyrard purposes to remove in a very ingenious way, by furnishing each of his mirrors with an apparatus not very complex, which we shall proceed to describe.

A small telescope, supported on a stand, and furnished with two wires, crossing each other in the focus of the glasses, may easily be directed to the point to which the image is to be conveyed. In this direction it is fixed by two screws. This telescope, without changing its direction, is moveable on its axis between two collars, and can be kept in any position round this axis by another screw. On this telescope is fixed the mirror, which it carries with it when it turns round its axis; and which, independent of this motion, is capable of turning round another axis, perpendicular to that of the telescope. The telescope is to be turned on its axis until the axis of the mirror is perpendicular to the plane formed by the incident and reflected rays, and in this position it is to be fixed by a screw. Lastly, the mirror is to be turned on its axis till the reflected rays are parallel to the axis of the telescope; and then the image of the sun must strike the object at which the telescope points.

The two movements here mentioned are executed one after the other, and are capable of considerable precision. With respect to the first, when the axis of the mirror is perpendicular to the plane of the incident and reflected rays, the edge of the frame, which is perpendicular to the axis of the mirror, throws its shadow in a plane parallel to the incident and reflected rays, and consequently parallel to the axis of the telescope. This shadow, therefore, or the boundary of the light reflected from the mirror, will cut an index projecting from the telescope in a right line at the same distance from the axis of the telescope as the edge of the frame is.

Accordingly, this right line being traced on the face of the index, for executing the first motion, it is sufficient to turn the telescope on its axis, till the shadow of the frame coincides with the right line on the index; which may be done with considerable precision.

For the second movement, it is clear, that when the mirror is so placed as to have its reflected rays parallel to the axis of the telescope, if in the axis of the mirror, and close to the edges of the frame, a little line of the silvering be removed, the want of silvering will produce a shadow, that will fall on the middle of the right line of the index. This middle point being previously marked on the index, to execute the second movement it suffices to turn the mirror on its own axis, till the shadow of the unsilvered stroke falls on this point; which may be done with the same precision as the former movement.

Thus we see, that every person employed, however great the number, may direct the image he produces to the point assigned for the focus, without troubling himself about what is done by the others, and without being disturbed by their operations. It may be observed too, that the motion of the sun in its diurnal axis is not so rapid, but that one person might attend to ten mirrors near each other, and keep them in the right position, which would greatly diminish the trouble and expense of the process.

We are of opinion, therefore, that Mr. Peyrard has carried the construction of burning mirrors, composed of several plane mirrors, to a degree of perfection that it had not before acquired, and appears to us to merit the approbation of the class.

Done at the *Palace of the Arts*, *3d of August*, 1807.

CHARLES,
ROCHON,
MONGE, *Reporters.*

The class approves this report, and adopts its conclusions.

DELAMBRE, *Perpetual Secretary.*

Paris, *August* 4, 1807.

Report of the state of Health and Disease in London during the Year 1809.

The experience of the last century has contributed to throw much light on the subject of epidemic diseases, which had been long treated upon gratuitous and hypothetical principles; and the numerous facts, which have been successively ascertained, although they do not amount to a complete elucidation of the whole phenomena of epidemic constitutions, yet they have removed much of the obscurity which involved our inquiries, and afford us a tolerably distinct view of the path which lies before us. Accordingly, the language of the writers of medical ephemerides has now assumed a more precise and definite form: the occult and supposititious qualities of the atmosphere are less frequently appealed to; but its obvious variations, and its sensible influence, partly on the living body, and partly on the various conditions of soil, from which many exciting causes of disease exhale, are justly deemed sufficient to account for those prevalent maladies, which the accidental importation or dissemination of a specific contagion, the scarcity or depraved state of provisions, the occurrence of some general panic, or the excessive fatigue and labour of large bodies of men, as in battles or sieges, do not enable us to explain. The ample historical detail of facts with which we prefaced our report for the year 1808, seems to afford this inference beyond all controversy. We there traced the coincidence of the most frequent and fatal epidemics in large cities, (of which London is a striking example) and the existence of certain conditions of the soil and superficies of the ground on which they stand; a coincidence which is always observable in the epidemic or endemic diseases, which generally occur in camps, when they are for some time stationary, or situated in low and damp situations. Following the course of improvement, by which the imperfections of a stationary camp have been gradually done away, and the paving, draining, and general cleanliness, adopted by the police and by individuals, have prevented the accumulation of a stratum of animal and vegetable matters, constantly collecting and suffering decomposition, in and around large cities; we then found that the plague began to cease from its ravages, that the remittent fever (which " raged like a plague," according to Burnet) next was mitigated in its frequency and fatality, that intermittent fever likewise began to diminish the number of its victims, and that malignant continued fever became less prevalent and less destructive among the familes of the poor. We found, that from the commencement of the general improvements in London and Westminster in 1762, the remittents and intermittents had almost disappeared; that dysentery was but an occasional occurrence; and that, from this period the annual mortality was gradually diminished, notwithstanding the rapid and extensive increase of the population in the out-parishes; so that the annual average of deaths is at present considerably smaller than it was a century ago, and less by several thousands than it was about the middle of the past century.

The seasons themselves, however, have undergone little or no change, as we proved from the testimony of medical writers of the 17th century, especially of the elegant Claromontius. The same vicissitudes of temperature, and the same severity of the vernal months were then the topics of complaint as at present. Yet we now find that the order of the seasons, in respect to the production of disease, is nearly reversed; although until those improvements in the structure and economy of the metropolis were made, the ancient character of the seasons continued to be correct: "Saluberrimum ver; autumnus longe periculosissimus."

The inference from these data, for the detail of which we must refer to our former volume, is obviously this; that the "constitution of the atmosphere," as to heat and cold, dryness and moisture, which the changes of season occasion, is not the source of epidemic diseases: and that the alterations in the condition of the air, which formerly rendered it the pregnant cause of disease and death, especially in the autumnal months, were not any occult intestine changes, not any general contamination brought from afar; but an impregnation received upon the spot, a poisonous exhalation which that portion of the air, immediately encompassing us, drank up from our own impurities. Hence, while the air was pestilential in the city or near the swamps, it was the breeze of health in the village and on elevated ground: and Dr. Heberden satisfactorily proved, that, in proportion as the cities of Europe were late or early in adopting the improvements in structure and police, to which we have alluded, the pestilence continued to visit them for a longer or shorter period.*

In these temperate latitudes then, experience has proved, that if, by removing the impurities of the soil, we deprive the atmosphere of those adventitious qualities, which it thence acquired during the hot seasons, and upon which its deleterious "constitution" entirely depended, the air at those seasons is actually most conducive to health. From this view of the subject we readily perceive the source of an immense diminution of mortality, and, of course, of a much greater diminution of disease; and can readily account for the subtraction of eight or ten thousand from the annual bill of deaths within the last eighty years, notwithstanding the great increase of the population of the out-parishes of London. Deprived of its adventitious properties, and capable only of affecting us by its sensible qualities, its heat and cold, dryness and moisture, &c. the atmosphere inflicts the greatest portion of disease in those months in which its temperature is lowest and most variable; viz. in the spring and winter, from January to April inclusive. But formerly these very seasons were the most healthy, or perhaps we ought to say, the least sickly. Yet, even in the most healthy season, the spring, intermittent fevers were generally very prevalent, although less general and less fatal than those of autumn. So that we may certainly conclude, that the most unhealthy season, under our present improvements of public and private economy, is considerably less unhealthy than the most favourable which our ancestors knew; the *saluberrimum ver.* And when we reflect on the multitudes of disorders which the autumnal exhalations generally propagated; and contrast this condition with our own; when even the disorders of the "saluberrimum ver" are greatly diminished—the extraordinary and universal amelioration of the public health in our own time becomes most manifest; and we cannot be surprised that in the years 1740 and 1741 upwards of 30,000 deaths should be recorded in the bills of mortality, while, in the past year, 1809, only 16,680 are registered, the smallest number that has occured in any year since the close of the 17th century.

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* Observations on the Increase and Decrease of particular Diseases, p. 84.

We noticed, in our last report, an observation of Dr. Heberden, that the annual mortality had increased from the beginning of the 18th century to the middle, and that it was at its greatest height from 1720 to 1750, from which time it gradually decreased. It might be observed also, that the number of births, which had increased from the beginning till about 1730, began again to decline, and was lowest in the middle of the century, from 1740 to 1760, from which time it has gradually increased. The average annual number of christenings, which had been 15623 in the first ten years of the century, became 17111 in the second decade, and 18203 in the third; but in the fourth decade it was only 16830, and in the fifth and sixth it was 14458 and 14789 respectively. Now, as the births were at the lowest number, while the mortality was at the highest, it is obvious, that not only the increase of the population of the metropolis, but a very large proportion of the ordinary number of inhabitants must have been then supplied by emigrants from the country. The average excess of the deaths over the number of births, which, in the third decade of the 18th century, viz. from 1720 to 1730, had been 9289, and in the fourth, 9962, amounted in the fifth to no less than 10894. So that, from the year 1740 to 1751, a supply of nearly *eleven thousand* persons from the country *annually* was required to support the population of London at the same standard. But since those improvements have been made, to which we before adverted, the annual number of births has gradually increased, on the one hand, while that of the deaths has more rapidly diminished; insomuch, that, before the close of the century, the births had, in three different years, exceeded the deaths. From 1760 to 1770 the average excess of the deaths above the births was 6265 annually; from the latter to 1780 it was 4290; from 1780 to 1790 it was 1407; and in the last decade it amounted to only 936: but in the years 1790, 1797, and 1799 the births exceeded the deaths by the numbers 842, 1631, and 836 respectively. Since the commencement of the present century the average is still more favourable to the population, the births having greatly exceeded the deaths every year except two, namely, 1801 and 1808; and the average excess of the births above the deaths during the nine years of the present century already elapsed is 1348 annually. In the past year, 1809, which may perhaps be considered, on the whole, as one of the most healthy years, of the mortality of which we have any record, the excess of the births above the deaths amounts to little short of 3000; the decrease in the number of burials, compared with those of the preceding year, is considerably above 3000; and the total mortality is less than in any year since the city was rebuilt after the great fire. We here subjoin the bill of mortality for the year.

The Diseases and Casualties in the Year 1809.

Abortive and Still-born	514	Jaundice	26
Abscess	49	Jaw Locked	4
Aged	1251	Inflammation	511
Ague	4	Influenza	3
Apoplexy and Suddenly	203	Livergrown	21
Asthma and Phthisick	488	Lunatick	166
Bile	2	Measles	106
Bleeding	24	Miscarriage	2
Bursten and Rupture	15	Mortification	167
Cancer	55	Palsy	123
Childbed	123	Palpitation of the Heart	1
Colds	15	Pleurisy	19
Colick, Gripes, &c.	15	Quinsy	3
Consumption	4570	Rheumatism	2
Convulsions	3463	Scarlatina	1
Cough and Hooping Cough	591	Scurvy	4
Cramp	2	Small pox	1163
Croup	81	Sore Throat	7
Diabetes	1	Sore and Ulcers	5
Dropsy	736	Spasm	24
Evil	2	St. Anthony's Fire	2
Fevers of all kinds	1066	St. Vitus's Dance	1
Fistula	3	Stoppage in the Stomach	20
Flux	9	Stranguary	1
French Pox	29	Teeth	308
Gout	30	Thrush	39
Gravel, Stone, Stranguary	10	Tumour	1
Grief	5	Water in the Chest	11

Water in the Head	252	Fright	1
Worms	5	Frozen	1
——			
Bit by a Rattle-Snake	1	Killed by Falls and several other Accidents	68
Bit by a mad dog	1	Killed themselves	52
Bruised	5	Murdered	1
Burnt	30	Overjoy	1
Drowned	124	Poisoned	4
Excessive Drinking	7	Scalded	5
Executed	6	Smothered	1
Found Dead	8	Starved	1
Fractured	2	Suffocated	7

Christened	Males	9981	In all 19612.
	Females	9631	
Buried	Males	8636	In all 16680.
	Females	8044	

Whereof have died,

Under two years of age	4937	Fifty and sixty	1419
Between two and five	1916	Sixty and seventy	1235
Five and ten	754	Seventy and eighty	1063
Ten and twenty	566	Eighty and ninety	369
Twenty and thirty	1145	Ninety and a hundred	54
Thirty and forty	1472	A hundred	2
Forty and fifty	1748		

Decreased in the burials this year 3274.

There have been executed in the city of London and county of Surry 16; of which number 6 only have been reported to be buried (as such) within the bills of mortality.

[*Annual Medical Review.*

Report of the state of the Hospital de la Charité.

		Discharged.	Dead.	Remaining, April 1st.
Patients in the Hospital of La Charité on January 1, 1809	104			
Admitted in January	146	108	28	
———— February	77	88	19	
———— March	115	95	18	86
Total	442	291	65	86

The mortality in this hospital is, therefore, on an average, upwards of one fifth of the whole number admitted; or, as M. Fouquier observes, it was one fifth in January, one fourth in February, and one sixth in March. We understand that the average mortality in the London Hospitals is about one in ten of those admitted; a fact which demonstrates the superior degree of cleanliness and ventilation adopted in them, if not the superiority of medical management. In the Royal Infirmary, at Edinburgh, we believe, the mortality is about one in fourteen of the whole; being one in eight or nine in the clinical wards, in which the most severe cases are selected, and one in twenty in the other wards.

The following is the catalogue of the diseases and their terminations in the Hospital de la Charité during the three months alluded to.

Diseases.	Admitted.	Cured.	Relieved.	Dead.	Remain.
Intermittent Fevers	81	52	7	—	22
Pulmonary Consumption	27	—	5	12	10
Chronic Catarrh	28	—	16	—	12
Aneurism of the Heart	24	—	10	9	5
Catarrh (acute)	22	17	1	—	3
Rheumatism (musculaire)	17	13	1	—	3
——— (articulaire)	9	5	4	—	—
——— chronic (vague)	3	—	1	—	—
Continued gastric Fevers (gastriques)	30	28	—	—	2
Fever, putrid (ou adynamique)	16	—	—	9	—
——, malignant	7	—	—	7	—
——, inflammatory, (ou angioténique)	4	4	—	—	—
——, bilious	2	2	—	—	—
——, slow nervous	1	—	—	1	—
Peripneumony	12	7	—	4	1
Pleurisy (acute)	8	4	—	1	3
——— (chronic)	2	—	—	2	—
Pleurodyne	2	1	—	—	1
Hæmoptysis	4	1	—	2	1
Inflammation of the tonsils	7	3	—	—	4
Schirrhus of the stomach	6	—	3	2	1
Pyrosis	1	1	—	—	—

Diseases.	Admitted.	Cured.	Relieved	Dead.	Remain.
Spasmodic vomiting	1	1	—	—	—
Periodic pain of the Stomach	1	1	—	—	—
Jaundice	6	5	1	—	—
Abdominal obstructions (embarras gastriques apyrétiques)	10	6	—	—	4
Colic (from metallic poison)	16	13	—	—	3
Diarrhœa	7	5	1	—	1
Dysentery	3	1	—	1	1
Peritonitis (chronic)	2	2	—	—	—
——— (acute)	1	1	—	—	—
Erysipelas of the face	1	1	—	—	—
Measles	1	1	—	—	—
Paralysis	9	6	2	1	—
Palsy of the tongue	6	2	2	1	1
Apoplexy	2	—	—	2	—
General Dropsy	2	1	—	—	1
Ascites	6	1	—	—	—
Hydrothorax	1	—	—	1	—
Anasarca	3	1	1	—	1
Indeterminate or feigned diseases	7	2	—	—	5
Sciatica	3	—	1	—	2

Medulla Spinalis.

Mr. William Sewell, of the Veterinary College, discovered, some years since, a canal in the medulla spinalis of the horse, bullock, sheep, hog, and dog. Upon tracing the sixth ventricle of the brain, which corresponds to the fourth in the human subject, to its apparent termination, he perceived the appearance of a canal, continuing by a direct course into the centre of the spinal marrow. Upon close examination, he finds its diameter large enough to admit a large sized pin; from which, by incision, a small quantity of colourless fluid issues like that contained in the ventricles of the brain. The canal is lined by a membrane resembling the tunica arachnoidea, and is situated above the fissure of the medulla: it extends as a continued tube through the whole length of the spinal marrow; and a free communication of the limpid fluid which the canal contains is kept up between the brain and whole extent of spinal marrow.

Injuries of the Head.

Two very interesting cases, and which, from their practical importance, merit detail, have occurred since our last. In the first an extensive separation of the pericranium was discovered only a few days before the patient's death, (a young healthy labourer). His symptoms denoted some serious affections of the head; but the injury, which had been received some months before, and was extremely slight, either escaped observation or was concealed, although inquiries were continually directed to it. He never referred to a local pain. On examination after death, an irregular piece of bone, as large as a crown, was found dead and discoloured, scabrous on both surfaces, and etched round the line of absorption; matter adhered to both the membranes, which were just separated to the same extent. It crossed the occipital ridge corresponding to the point where the longitudinal falls into the lateral sinuses.

The second case was that of a man aged 50, who received a blow with a brick-bat on the occipit on the 27th of September, for which, though he was not inconvenienced, he got bled and purged, and the little wound of the scalp was dressed, which healed in a fortnight. On the 20th of the following month he was observed to talk incoherently, to lose his self possession, from which he gradually sunk into a state of stupor and died. On raising the dura mater an oval pellucid tumour presented itself on the middle of the outer side of the left hemisphere of the brain. The bone and all the membranes were perfectly sound. The disease extended through the substance of the hemisphere, and presented other very singular and unnoticed appearances, which must be reserved for a fuller opportunity of description. *London Medical Review.*

Cases of Aneurism.

Mr. Ramsden, of St. Bartholomew's Hospital, has lately tied the subclavian artery above the clavicle, in a case of axillary aneurism, which had not materially altered the site of the clavicle. The operation, we are sorry to add, was unsuccessful. We refrain from particulars, being informed that the operator designs to make them public.

A highly interesting example of the spontaneous cure of a large subclavian aneurism has occurred in the same hospital. The tumour is now not so large as a small pullet's egg, hard and incompressible. The clavicle has been partially absorbed by its pressure. No pulsation can be felt below this bone; and the pulse of the corresponding wrist is weaker than of the opposite. The communicating vessels cross the tumour towards the back of the scapula, and look like pulsating veins. They can be almost traced to their origin from the thyroidea inferior.

An axillary aneurism as large as a plumb, produced by the section of the artery about eight months ago, in a little boy, has likewise gradually worked its own cure.

We are gratified to have it in our power to announce the complete success of the operation stated in our 7th number to have been performed for an aneurism by anastamosis in the orbit. The tumour has become solid and shrunk to less than half its magnitude, and the pulsation has entirely ceased. *Ibid.*

Case of Ruptured Bladder.

A healthy middle aged gentleman, after drinking two bottles of wine at a dinner party, made ineffectual efforts to pass urine. Soon after his return home he was found senseless on the floor of his chamber. He had fallen from the excruciating pain he suffered in the attempt to make water. On passing a catheter, only a few spoonfuls of pale urine escaped; and the operation was attended by intolerable pain. Marks of abdominal inflammation soon presented themselves, and the case was very actively treated accordingly. The symptoms commenced on the evening of the 21st of October, and he died on the evening of the 23d.

Dissection. General blush upon the omentum and peritoneum, most apparent on the small intestines, effusion of coagulable lymph commencing, adhesions not yet established. Half a gallon or more of fluid in the peritoneal sac, having a strong odour of wine. A laceration of the fundus of the bladder posteriorly, large enough to take three fingers. Rent of the peritoneal coat extended quite across the bladder. Lips of the wound rough and lived—bladder but slightly inflamed. *Ibid.*

Case of Amputation of the Shoulder Joint.

An extosis, of three years growth, which had acquired extraordinary magnitude, spreading over the neck and part of the body of the os humeri, was removed by Mr. Cooper, in Guy's Hospital, on the 11th of this month. The operation was an amputation at the shoulder joint. The patient, a woman of thirty, is doing well. The tumour weighs from ten to eleven pounds, and is partly cartilaginous. *Ibid.*

Case of Ruptured Urethra.

Mr. Soden, of Coventry, has communicated to us a case of retention of urine, arising from a neglected stricture, in which the urethra gave way between the obstructed part and the bladder. The urine escaped among the cellular membranes, and very alarming symptoms rapidly supervened. In the course of four or five hours the scrotum and penis were distended to an enormous size; there was a considerable tumefaction above the pubis, and a line of inflammation across the lower part of the abdomen, marked the extent of the diffusion in that direction. An opening was made into the urethra through the most anterior part of the perinæum. A flexible catheter was introduced through the wound into the bladder, and secured in that situation. Nearly two quarts of urine were discharged through the canula; the bladder was afterwards evacuated by withdrawing the stilet, as occasion required. The stricture was situated about four inches down the urethra. It has been relieved by the application of caustic, and the greater part of the urine passes through the penis. The wound is reduced to a very small fistulous orifice, which has resisted the means that have hitherto been employed for its obliteration.

The result of the case shall be given in a future number.

Ibid.

Case of Diabetes Mellitus.

Dr. Laird has lately had under his care, at the Public Dispensary, a patient labouring under diabetes mellitus, in whom the plan of depletion by the lancet, recommended by Mr. Watt in his recent publication, was employed as the principal method of treatment. The patient, thirty years of age, had been subject to the disease for about ten months. In five successive bleedings seventy ounces of blood were taken away. Although the result of this practice is imperfect, as, notwithstanding the increase of strength, general improvement in appearance, and diminution of the quantity of urine and of the quantity of saccharine extract, the disorder still continued, when the patient ceased to be under Dr. Laird's superintendance; yet we are induced to notice this case, in consequence of its tending to establish the safety of the use of the lancet in this formidable malady; and from a hope that the safety being established, it may have the benefit of a fair trial by an extensive employment.

Ibid.

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